Phyſico-Mechanical
EXPERIMENTS
ON
Various SUBJECTS.

Containing an Account of

Several Surprizing *Phænomena* touching *LIGHT* and *ELECTRICITY*, producible on the Attrition of Bodies. With many other Remarkable Appearances, not before obſerv'd.

Ex Together with Lib

The Explanations of all the *Machines*, (the Figures of which are Curiouſly Engrav'd on Copper) and other *Apparatus* us'd in making the EXPERIMENTS.

Dab: To which is Added, verner 1728

A *SUPPLEMENT*, containing ſeveral *New Experiments* not in the Former Edition.

The SECOND EDITION.

By *F. HAUKSBEE*, F.R.S.

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THE PREFACE.

THE Learned World is now almoſt generally convinc'd, that inſtead of amuſing themſelves with *Vain Hypotheſes*, which ſeem to differ little from *Romances*, there's no other way of *Improving* NATURAL PHILOSOPHY, but by *Demonſtrations* and *Concluſions*, founded upon *Experiments* judiciouſly and accurately made.

By this courſe, after many Ages had paſs'd, with little or no Progreſs in the *True Knowledge* of the *Nature of Things*, greater Advances have been made within the compaſs of a ſmall Number of Years, than was eaſily to be imagin'd, that the moſt Sagacious Men, with their Greateſt Induſtry, could ever have been capabable of attaining to.

The Honourable and moſt Excellent Mr. *BOYLE*, by great Variety of Experiments, in almoſt *every Part of Philoſophy*, gave much Light into the Cauſes and Operations of Nature; and particularly, by the Invention of that moſt Uſeful Inſtrument the *Air-Pump*.

The Principal Subject of the following Papers, is, an Account of *Great* and *Further Improvements* of this Noble Machine, the *Air-Pump*, and of many New Experiments made thereby.

By the ſame Method, the moſt Learned and Incomparable Sir *ISAAC NEWTON* has invented and eſtabliſh'd the Theory of *Light* and *Colours*; and by Demonſtrations founded on Experiments and Obſervations, has at once begun and finiſh'd that great Diſcovery, and advanced that Part of Opticks, concerning the Nature of *Light* and *Colours*, of which there was little, if any thing, before known, to a *Perfect* and *Compleat Science*.

The *New Experiments* contain'd in the Following Treatiſe, concerning the Production and Emiſſion of certain Kinds of *Light* from different Bodies, hitherto unobſerv'd, may, 'tis preſum'd, give no ſmall Illuſtration to that Matter; and become the Occaſion of many not unacceptable Diſcoveries, concerning ſeveral particular Circumſtances not included in the General Theory.

The PREFACE.

The General Laws of *Attraction* and *Repulſe*, common to all Matter, have by the ſame Excellent Perſon been diſcover'd, and applied to Wonderful Purpoſes, in eſtabliſhing the true Syſtem of Nature, and explaining the Great Motions in the World.

But the *Nature* and *Laws* of *Electrical Attractions* have not yet been much conſider'd by Any: And in the following Obſervations, 'tis hoped, the Reader may meet with many things, which may be of great Uſe in diſcovering ſome of the Wonderful and hitherto Unheeded Effects of this ſtrange Property of Bodies, in ſeveral of the Operations of *Nature*; and poſſibly in the Production and Determination even of *Involuntary Motions* in the *Parts of Animals*; of which very little has yet been wrote intelligibly.

If

The PREFACE.

If the few Hints and Suggeſtions in this Diſcourſe, ſhall excite the Curioſity of *Ingenious Inquirers*, to make further Search into theſe Matters, my intention in publiſhing them, which was entirely for the *Improvement* of *Natural Knowledge*, will be compleatly anſwer'd.

F. H.

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Physico-

Phyſico-Mechanical EXPERIMENTS, &c.

A General Deſcription of the AIR-PUMP *made uſe of in the following* Experiments.

THE *AIR-PUMP* (delineated Plate I.) conſiſts of two Braſs Barrels or Cylinders, as repreſented by *a a a a*, twelve Inches in height, and two their Diameters within. The Suckers, or *Emboli*, are raiſed and depreſs'd by turning the Winch *b b* backward and forward. The Winch is faſten'd to a Spindle, that paſſes through a a Lanthorn, whoſe Pins perform the Office of Cogs: For in its Motion they lay hold on the Teeth of the Racks *c c c c*, and ſo reciprocally as one is depreſs'd, the other is elevated: By which means the Valves, which are made of limber Bladder, and fix'd on the upper Part of each *Embolus*, as well as at the bottom of the forementionꝛd Cylinders, perform their Offices mutually of exhauſting and diſcharging the ſame Air taken from the Recipient on the Plate of the Pump. And when the Recipient comes to be pretty well exhauſted of its contain'd Air, the preſſure of the outward Air on the deſcending Sucker is nearly ſo great, that the Power requir'd to raiſe the other is very little more than what ſurmounts the Friction of the moving Parts; which renders this Pump preferable to all others: For in the Working of them, the nearer they approach to a *Vacuum*, the greater is their Labour. But this that I am now deſcribing (under the ſame Circumſtances) is quite contrary.

The bottom of the Barrels are placed in a Braſs Diſh, repreſented by *dd*, whoſe Sides are about two Inches high, and is on purpoſe to put Water in, to keep the Leather Collars (on which the Braſs Cylinders ſtand) moiſt, whereby the Air is prevented from inſinuating into the Cylinders of thoſe Parts.

The Cylinders are ſcrew'd down on the ſame by the Nuts *eeee*, which force the Frontiſpiece *ff* down on them, thro' which the Two Pillars *gggg* paſs.

The Pillars have an Iron belonging to each of them, and paſs from them in the Form of a Swan-neck, decypher'd by *gg*, which Irons are faſtned to the hinder part of the Frame, for their better ſecurity from ſhaking.

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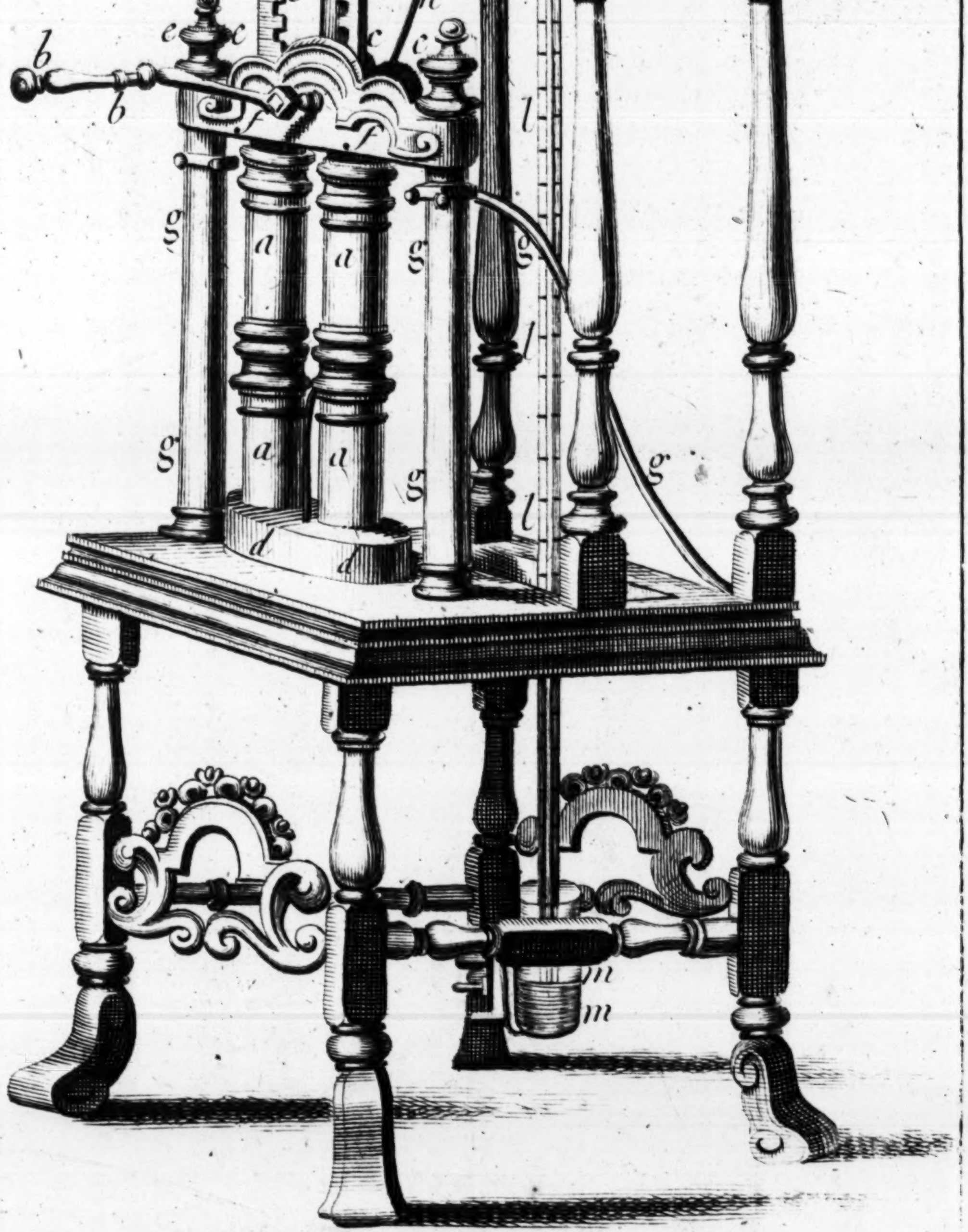
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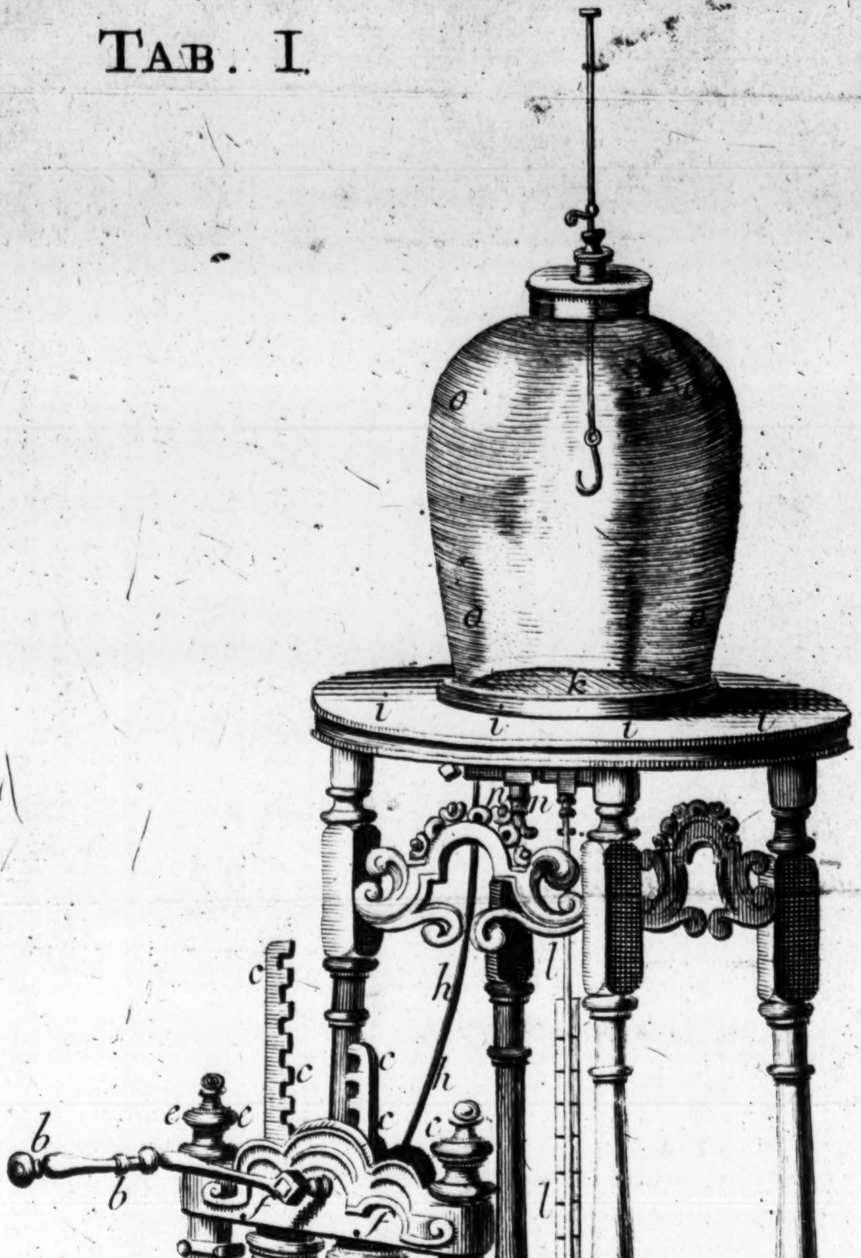
TAB. I.



From between the two Braſs Barrels ariſes a Braſs hollow Wire, *hhhh*, which hath a Communication with each of them, by means of a perforated Piece of Braſs which lies along horizontally from one to the other.

The upper end of this hollow Wire is faſtned to another Piece of perforated Braſs, which ſcrews on underneath the Plate *iiii*, which is ten Inches over, and has a Braſs Rimm ſoder'd on it, to prevent the ſhedding of Water; for which there is occaſion in ſeveral Experiments. Between the Middle and the Side of this Plate ariſes a ſmall Pipe, *k*, about an Inch and half in height, through which into the forementioned hollow Wire paſſes all the Air into the Barrels, as it is taken from the exhauſting Receiver. Upon the Plate of the Pump is always laid a wet Leather, on which the Recipients are placed: This wet Leather, prevents the Air's getting into the Glaſſes, whoſe Edges are truly ground, and is of uſe for that purpoſe beyond any Cement whatſoever; and not only ſecures it from the Air's Ingreſs that way, but by the Uſe of it we can make ſeveral Experiments in the ſame time they could formerly make one, without any Daubing or Difficulty.

Another Excellency in this Pump is, the Contrivance of the Gage denoted by *llll*; which Gage is a Glaſs Tube about 34 Inches long, and is ſo placed, that it cannot eaſily receive Damage, and is altogether out of the way of any thing that is experimented on the Pump. Its lower Orifice is plung'd in a Glaſs of Mercury deſcrib'd by *m m*, on the Surface of which is laid a Piece of Cork with a Hole in the middle of the Glaſs Tube to paſs thro': On this Cork is plac'd a Board made of Box-Wood, about an Inch in Breadth, and groov'd in the middle to receive the fore-mention'd Glaſs Tube, which is looſely loop'd on to the ſame by two Braſs Loops, that it may have the Liberty of riſing and falling as the Mercury aſcends or deſcends in the Gage.

To the upper part of this Tube is cemented a Braſs Head, which Braſs Head fits into the fore mentioned perforated Braſs Piece that is ſcrew'd on under the Plate, and has a Communication as well with the Recipient on the ſame, as with the hollow Braſs Wire *h h h h* paſſing between the two Barrels.

The Box Board is graduated into Inches and Quarters, from the Surface of the Quickſilver to 28 Inches high: From thence 'tis divided into Tenths of Inches. From this Gage the Degrees of Rarefaction in any Experiment are at all times moſt nicely to be obſerved.

The Air-Cock, *n*, which lets in the Air, is likewiſe a Screw on the ſame fore mention'd perforated Braſs, in which the upper parts of the Gage and hollow Wire are inſerted: *o o o o* repreſents a Receiver ſtanding on the Plate of the Pump, on whoſe upper part *p p*, through a Box of Collars of Leather, paſſes a Slip Wire, whoſe Office is to take up, let fall, or ſuſpend any thing at any determinate Height, in the Receiver, without the Air's Inſinuation.

SECT. I.

An Account of ſeveral Experiments on the Mercurial Phoſphorus.

EXPERIMENT I.

I Took a Glaſs Receiver, open and ground at both Ends, and capable of containing about thirty Ounces of Water: The upper Orifice of this Receiver was cloſ'd with a Braſs Plate, (by the help of a wet Leather laid on the Edge of it,) in the middle of which was ſcrew'd a Stopcock, that had a ſmall Glaſs Tube inſerted into the lower Orifice of it; the Inſertion was perform'd by means of a Cement: And the little Tube thus inſerted, reach'd nearly from thence to the Bottom of a Glaſs, which was alſo included within the aforeſaid Receiver, and which had as much Quickſilver in it as would cover the Bottom of the Tube about a quarter of an Inch. This *Apparatus* was then applied to the Pump, and the Stopcock turn'd, to hinder the Air's Paſſage that way, till the Receiver was ſufficiently exhauſted: Which done, the Stopcock was turn'd again, to

See Fig. 3.
Plate III.

to give the Air free liberty to enter in; and then the Air making its way thro' the Tube before mention'd, ruſh'd with a very great violence thro' the Body of the *Mercury*, blowing it up forcibly againſt the Sides of the Veſſel that contain'd it. And in this Confuſion and Hurry of its Parts, *it gave*, *all round*, *the Appearance of Fire*; it look'd like one great flaming Maſs, compoſed and made up of innumerable little glowing Balls, which being forc'd up, and daſh'd againſt the Sides of the Glaſs by the impetuous Torrent of Air, fell down again by their own Weight into the reſt of the *Mercury*. And thus the *bright* Phænomenon continued, till the Receiver was half fill'd again with Air.

The Reſult of this Experiment therefore, ſhews us, that *Light is producible from* Mercury, *by paſſing common Air through the Body of it, after the Receiver is well exhauſted: i e.* that *Light is producible by the Application of a very ſubtile and penetrating Mover, to a Fluid of great Denſity, whoſe Parts are moſt minutely divided, and of a ſmooth and poliſh'd Superficies, and placed where it has little Diſturbance, but from that Body which gives the Motion to its Parts.* For ſuch a *denſe* and *polite Body* is *Mercury*; ſuch a *ſubtile Mover* is the Air, and ſuch an *apt Repoſitory* is an Exhauſted Receiver.

Experiment II.

HAving provided a *Receiver* of about 21 Inches in height, I ſcrew'd to the upper Orifice of it a *Glaſs* reſembling thoſe now commonly us'd for Cupping, having an open Paſſage through its Neck, in which was cemented a Piece of a ſmall Tube, drawn tapering to one end by the Flame of a Candle: This, together with the Cup, made an entire *Funnel*, the ſmall Aperture of which was ſtopt with a round little Plug of Wood, to prevent the *Mercury*'s entring the Receiver before its due time. Within this tall Receiver was included a *Glaſs* of the height of about 17 Inches, which had a round Crown like a *Shade* (as they generally call thoſe Fences which are put over Images to keep them from the Duſt.)

See Fig. 3. *Plate* III.

This whole *Apparatus* thus ſet together, was plac'd on the *Pump*, and about a Pound and half of *Mercury* put into the Funnel; and then working the Pump, by that time the Air had been drawing out for the ſpace of two Minutes, there was enough exhauſted for exhibiting the Phænomenon intended. Having then looſen'd the Plug therefore, which ſtopt the Funnel, the Mercury was driven by the Preſſure of the

Air

TABU. II.

Fig: 3.

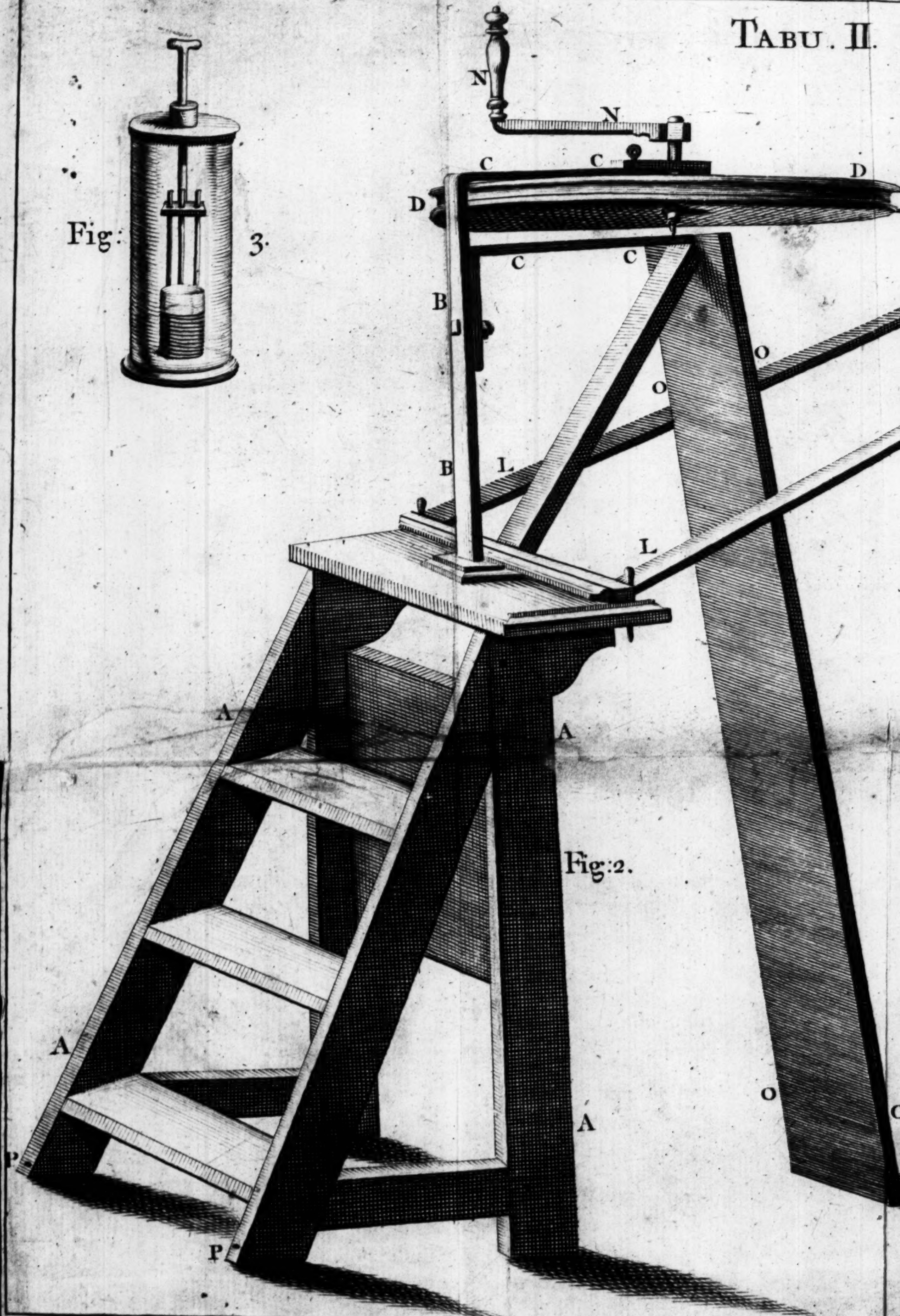


Fig:2.

BU. II.

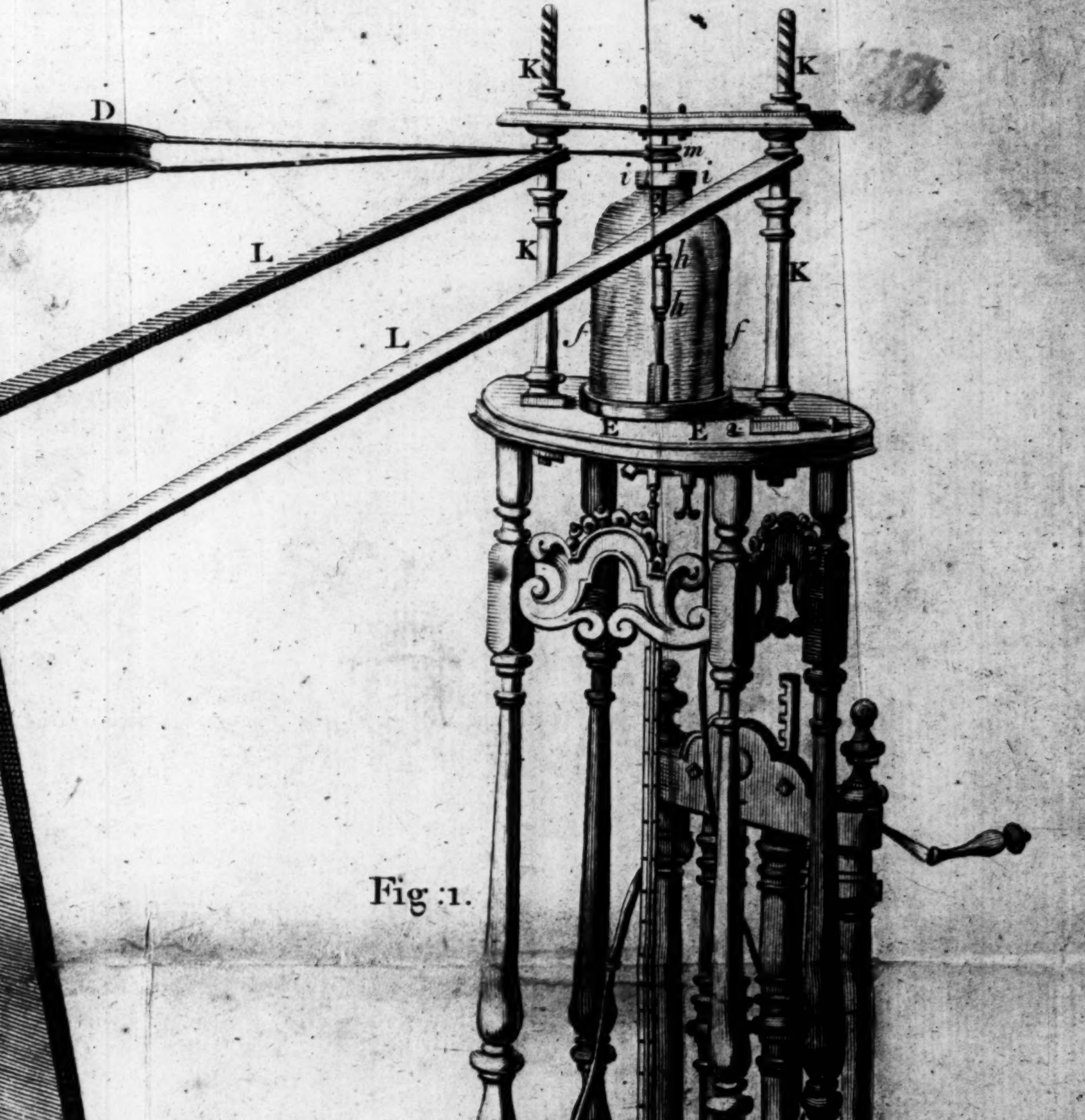


Fig :1.

Air with great Violence into the Receiver, and ſtriking forcibly on the Crown of the included Glaſs, was thereby broken into very ſmall Particles, and gave the ſurprizing appearance of a Shower of Fire deſcending all round the Sides of the Glaſſes. The Light it gave in its Deſcent, was ſuch, that the Form of the Receiver, and the Glaſs included therein, were both very diſtinguiſhible, and continued ſo to be, till all the Mercury had paſs'd through the Funnel. All that ſpace of time, I ſay, the Repreſentation laſted; neither could any thing more lively expreſs ſuch a fiery Shower, than this Deſcent of the Mercury in *Vacuo*.

What farther occurr'd to Obſervation in this Phænomenon, I think proper to take notice of in ſome Particulars by themſelves. I obſerv'd then,

That the Deſcent of the Mercury reſembled rather the Fall of Snow, than that of Rain, by reaſon of the Slowneſs of its Motion.

That none of it appear'd luminous, but what was contiguous to the Sides of the Glaſſes in its Deſcent.

That the Globules of Mercury deſcended ſome ſwifter than others, according to their different Magnitudes.

That the Mercurial Globules did not barely ſlide down upon the Sides of the Glaſſes, but were alſo turned about circularly ; or, in other Words, that beſides their *Motion of Perpendicular Deſcent*, they had alſo a *Circular one about their own Axes.*

That the Parts of the Mercury contiguous to the Glaſs, were by theſe Circumgyrations continually tearing and ſeparating from their Contact with the Glaſs ; and by that means were wrought up into ſuch a Form or Shape, as was proper for the Production of Light from ſuch a Body in ſuch a *Medium.*

That the ſmaller Globules, which adher'd to the Glaſs, and whoſe Weight was not ſufficient to cauſe their Deſcent, remained opaque ; For (in this, as well as all other Mercurial Experiments) no Light is to be obtained without Motion.

That the very ſame Motion as this was in *Vacuo*, given to the ſame Mercurial Globules in *Common Air*, will not produce the ſame Effect : Which I try'd, by condenſing Air ſtrongly on the Surface of the Mercury, and ſo forcing that Mercury thro' Leather.

The Reſult of all which Obſervations put together, is, That *a peculiar Figure and Motion of Parts, as well as a proper Medium for thoſe Motions to be performed in, are requiſite to the Production of the Mercurial* Phoſphorus.

Having ſince repeated this Experiment, and that with a *greater quantity* of Mercury; I have obſerv'd ſome particular Appearances in it, ſo remarkable and ſurprizing, that I thought I ought not to paſs 'em by in ſilence.

I made uſe of a quantity of Mercury, about *as great again* as what I us'd before, *viz.* near upon three Pounds: And now the deſcending Mercury did not only appear like a Shower of Fire, (which it did at the firſt Trial) but alſo the Light darted thick from the Crown of the included Glaſs, *like Flaſhes of Lightning*, of a very pale colour, and eaſily diſtinguiſhible from the reſt of the Light produc'd. Theſe Flaſhes I have obſerv'd to be darted ſometimes *Horizontally*, ſometimes *inclining upwards*, at other times *downwards*. And beſides this difference with reſpect to the *manner* of the *Reverberation* of the Flaſhes, there was another thing obſervable with reſpect to the *Quarter* from whence they were ſo reverberated; for they would be thrown not only from the *included* Glaſs, but ſometimes alſo from the *including* Receiver: And I have ſometimes ſeen them rebound into Figures ſo very odd and ſurprizing, that I have no Idea of any thing that can ſerve for a juſt Compariſon with 'em. But this is certain as to theſe ſtrange Flaſhes, that they have ſometimes ſeemingly *proceeded directly from the Stream of Quickſilver*, (as it deſcended from the Funnel) *before ever it* *reach'd the included Glaſs*; and that their general Courſe is, to *fly to the ſide of the outward Receiver*, where the Light breaks, and ſpreads it ſelf into theſe odd Forms. This is what they moſt frequently do. But if they take their Original *from the Side of the Receiver*, (as I have ſometimes obſerv'd) then their Courſe is different.

Laſtly, It deſerves notice alſo, that during all the time of the Mercury's deſcent thro' the Funnel, (which was at leaſt two minutes) *the Crown of the included Glaſs appear'd to be ſenſibly more enlightned than all the reſt of the parts of it:* And this Light was *uniform*, and without any alteration, as long as the Quickſilver kept running.

Experiment III.

IN the foregoing Experiments, we have ſeen the Production of the Mercurial *Phoſphorus*, in the fine and much rarefied Medium of a *Vacuum*; by which 'tis plain, that ſuch a Medium as that, is accomodate to the Nature of this Phænomenon, and will ſerve for the Exhibition of it; but we can by no means infer from thence, that no other Medium will do; or, that Light, which may be produc'd in the rarefied Medium of a *Vacuum*, may not alſo

TABU. III.

Fig:6.

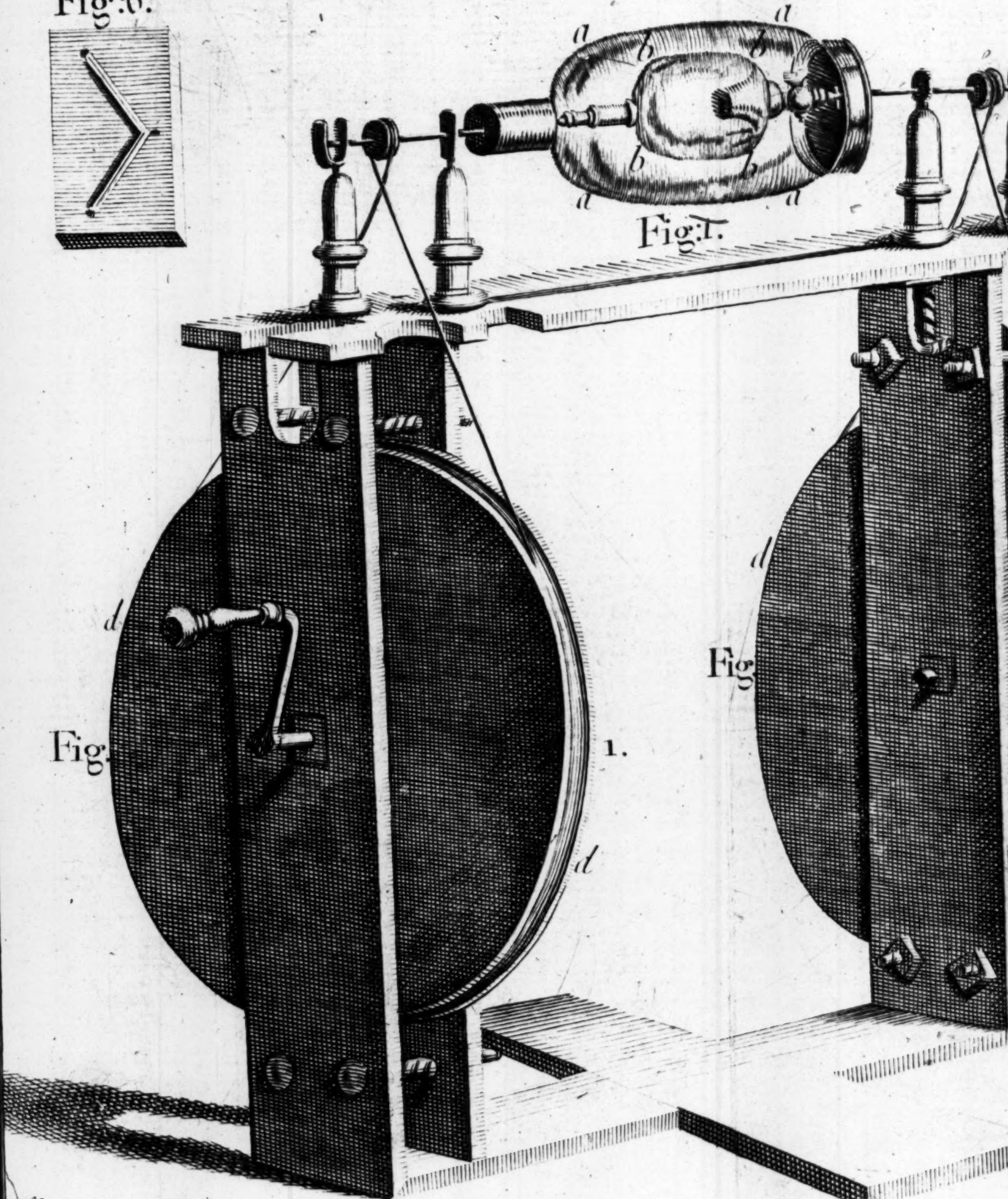
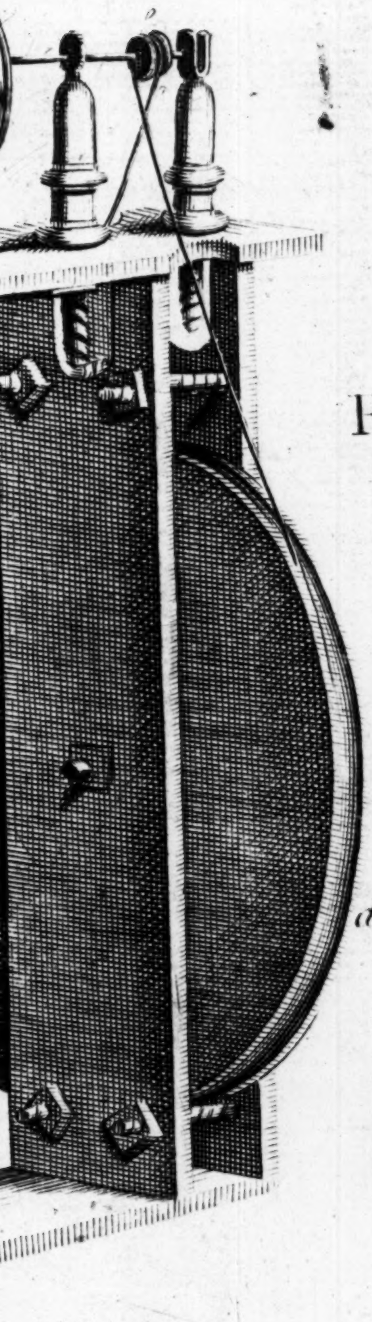


Fig:I.

Fig. 1.

Fig



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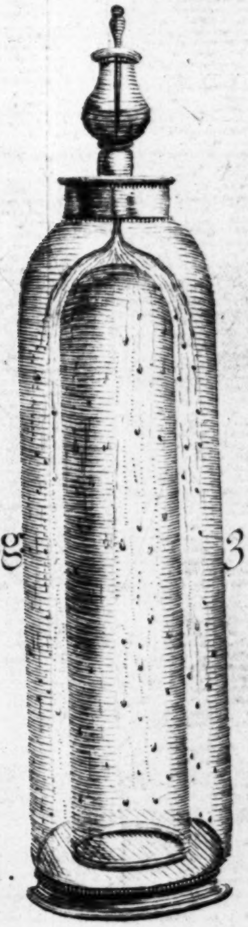


Fig. 3.

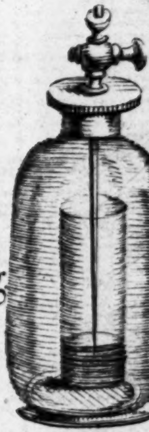


Fig. 2.

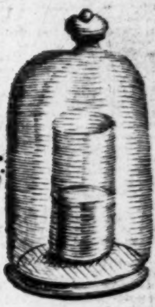


Fig. 4.

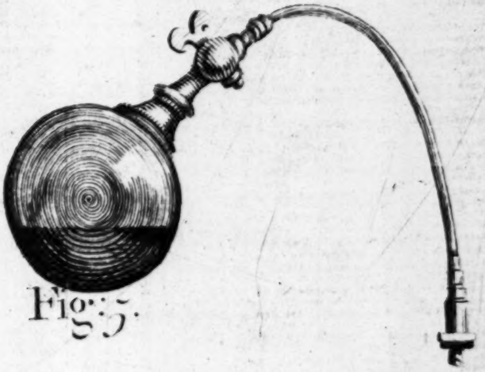


Fig. 5.

alſo be produc'd in a Medium leſs thin and rare than that. To bring this matter therefore to a determination, (*viz.* whether ſo thin a Medium as a *Vacuum*, or the neareſt approach to it, be abſolutely neceſſary to the Production of ſuch a Light, as is diſcoverable in the Barometer by putting the Mercury in motion,) I proceeded after the following manner:

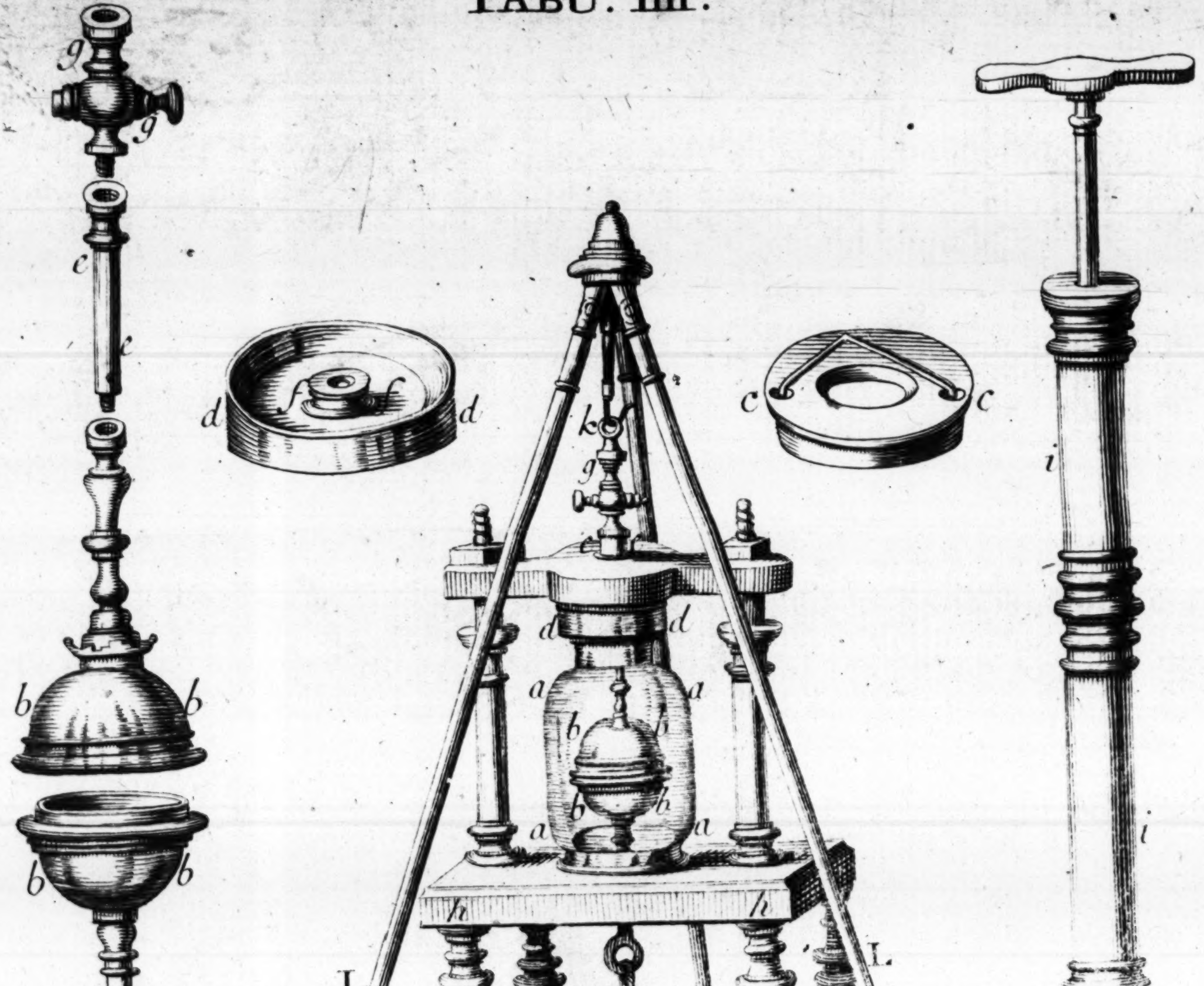
The *Mercurial Gage* (an Inſtrument now univerſally known) I concluded would be the moſt proper Inſtrument for this Diſcovery. Having therefore plac'd a ſmall *Receiver* upon the *Plate of the Pump*, the Air was exhauſted from it, till the Mercury in the Gage was elevated to 29 Inches and a Half; then ſuffering ſome Air to enter the Receiver by the *Cock*, the Mercury in the Gage deſcended, and made ſeveral Vibrations before it reduc'd it ſelf to a ſtate of reſt; and the Mercury having no other motion impreſs'd upon it in all theſe Vibrations, but barely that which the Air caus'd by its entrance, the Mercury all this time, tho' it did *appear luminous*, yet appear'd ſo only in the *Deſcents*, and not in the *Aſcents*: But when the Mercury came to be broken and divided by a violent agitation and ſhaking, then the broken parts appear'd *luminous* in *ſome part* of their Superficies, the *other part* always being *opaque*; and that after this manner: The *undermoſt* Superficies of the Mercurial Globules in their *Aſcent* became *concave*,

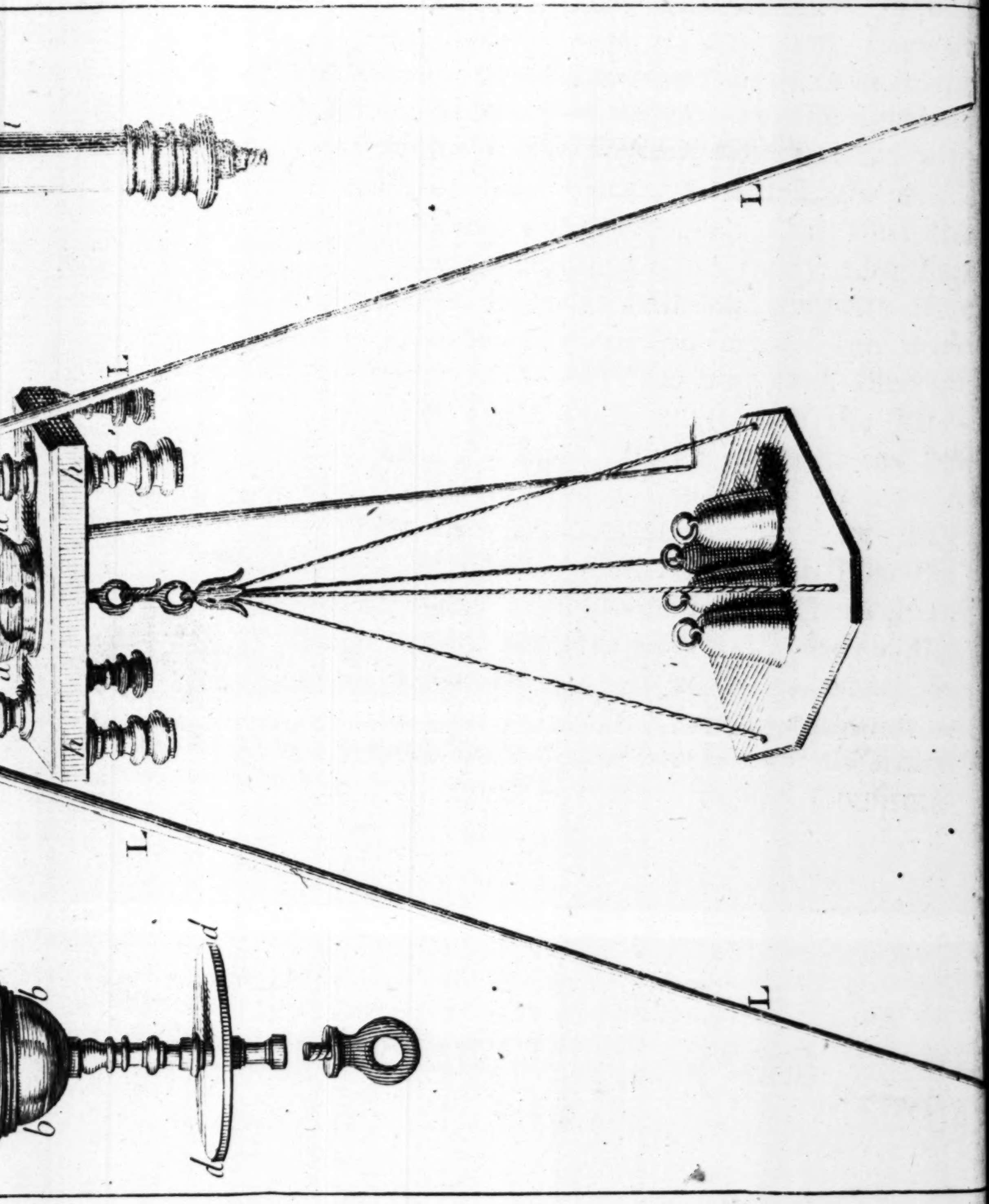
Aſcent,

and there they were *luminous*; but the *uppermoſt* Superficies of the ſame Globules, in their *Deſcent*, became *concave*, and there likewiſe *luminous*: But the *uppermoſt* Superficies in the *Aſcent*, which were *convex*; and the *lowermoſt* Superficies in the *Deſcent*, which were alſo *convex*, in both Places, gave no Light at all, but continued always *opaque*. And thus the Appearance continued upon every admiſſion of the Air, till near half the quantity that was exhauſted was return'd again: But after that quantity of Air was admitted, then no manner of Light would enſue, tho' the Mercury had the ſame motion given it as before.

From this Experiment therefore we may draw the following Concluſion, *viz.* That *tho' the Mercurial* Phoſphorus *in the* Torricellian *Experiment is not producible in ſo denſe a Medium as common Air, yet it by no means requires ſo thin and ſo much rarefied a Medium as that which makes a near approach to a Vacuity.* And this Truth receives a further Confirmaon by the following

TABU. IIII.

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Experiment IV.

I Provided ſome Quickſilver, very fine, and free from the leaſt Appearance of Soil on its Surface. The Glaſs alſo which contain'd it, was made very clean and dry. I included this Glaſs with its Mercury in a *Receiver* to the Plate of the Pump, and exhauſted the Air, till I found the Mercury in the *Gage* ſtanding at 28 Inches, and not above, (the Mercury in the Barometer at the ſame time being at 29 Inches and a half.) At this Elevation of the Mercury in the *Gage*, the Pump was ſhaken, and by that means the Quickſilver in the included Glaſs was put into motion. *The Effect* of this Concuſſion was *the appearance of a Light*, bright enough to render the Receiver and the included Glaſs plainly viſible and diſtinguiſhible; and not only ſo, but a Man's Hands and Fingers on the outſide might eaſily be diſcern'd likewiſe. This was the Principal Matter of Fact enquir'd after by the Experiment; but I obſerv'd farther,

See Fig. 4. *Plate* III.

That tho' the Light might be produc'd by a ſmall Agitation of the Mercury, yet that Light would be encreas'd by a greater and ſtronger Agitation.

That when a pretty briſk Motion was given to the Mercury, it would give the Repreſentation of Waves of Light, breaking on the Sides of the Glaſs, and ſcattering ſome Species of the ſame Appearance towards the upper Part of it.

That upon the Repetition of the Experiment, the *Phoſphorus* ſeem'd each time more vivid than the other; till at laſt, by often ſhaking the Quickſilver, its Surface became ſomething ſoil'd, and ſo the Light leſs than it had been before.

That (in this, as well as all other Experiments on the Mercurial *Phoſphorus)* the Light exhibited is of a very pale Colour.

That the firſt Appearance of the Light, is when about half the Air contain'd in the Receiver is exhauſted; and, That it ſtill encreaſes with the encreaſing Rarefaction of the *Medium.*

From this Account it appears, that the former Concluſion is again confirm'd, *viz. That there needs not the neareſt Approach to a Vacuum, to produce the Mercurial Phoſphorus.*

And theſe two laſt Experiments put together, make up the fulleſt Proof of it that can be deſir'd.

For in one of them the Air was *let in upon the Mercury* plac'd in the exhauſted Receiver, and a Light was produced that way: In the other, the Air was *not totally remov'd from the Mercury,* and a Light was produc'd that way alſo.

TAB. V.

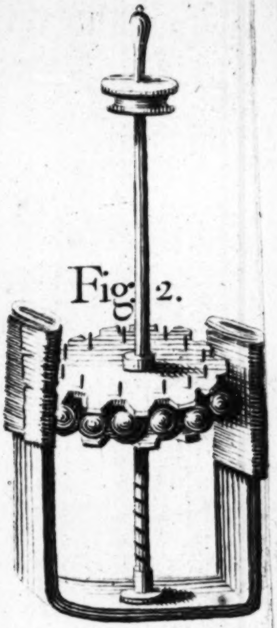

Fig. 2.

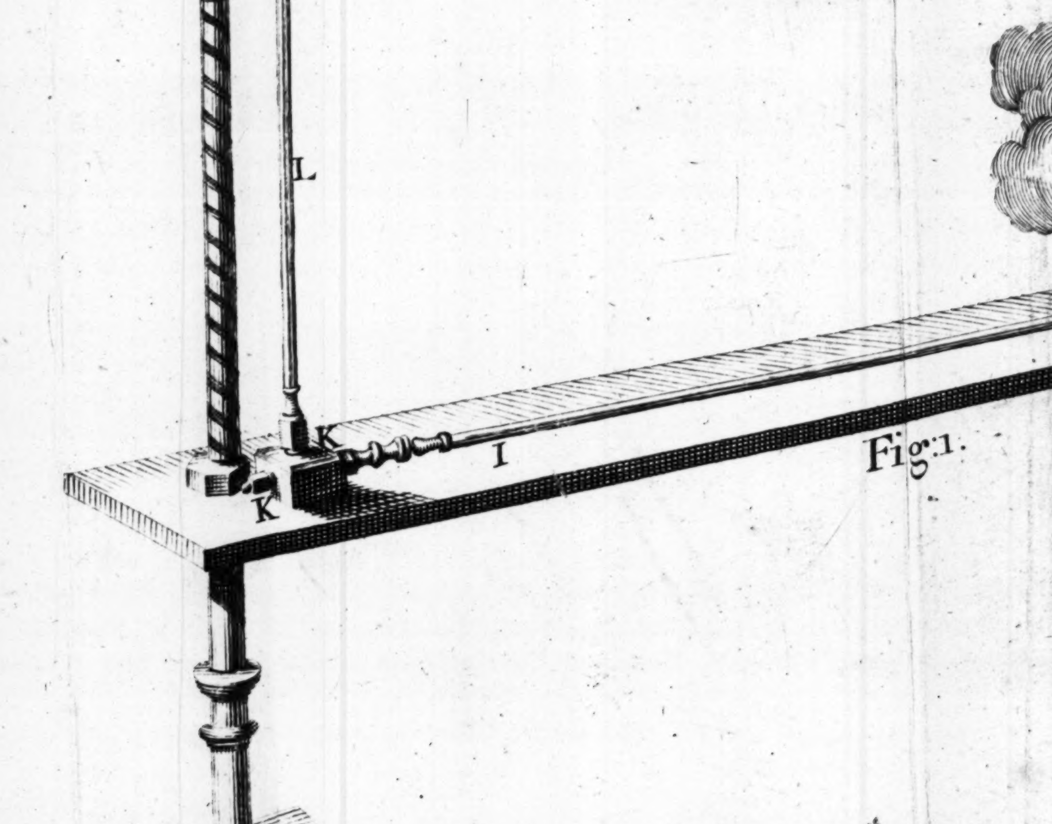

Fig. 1.

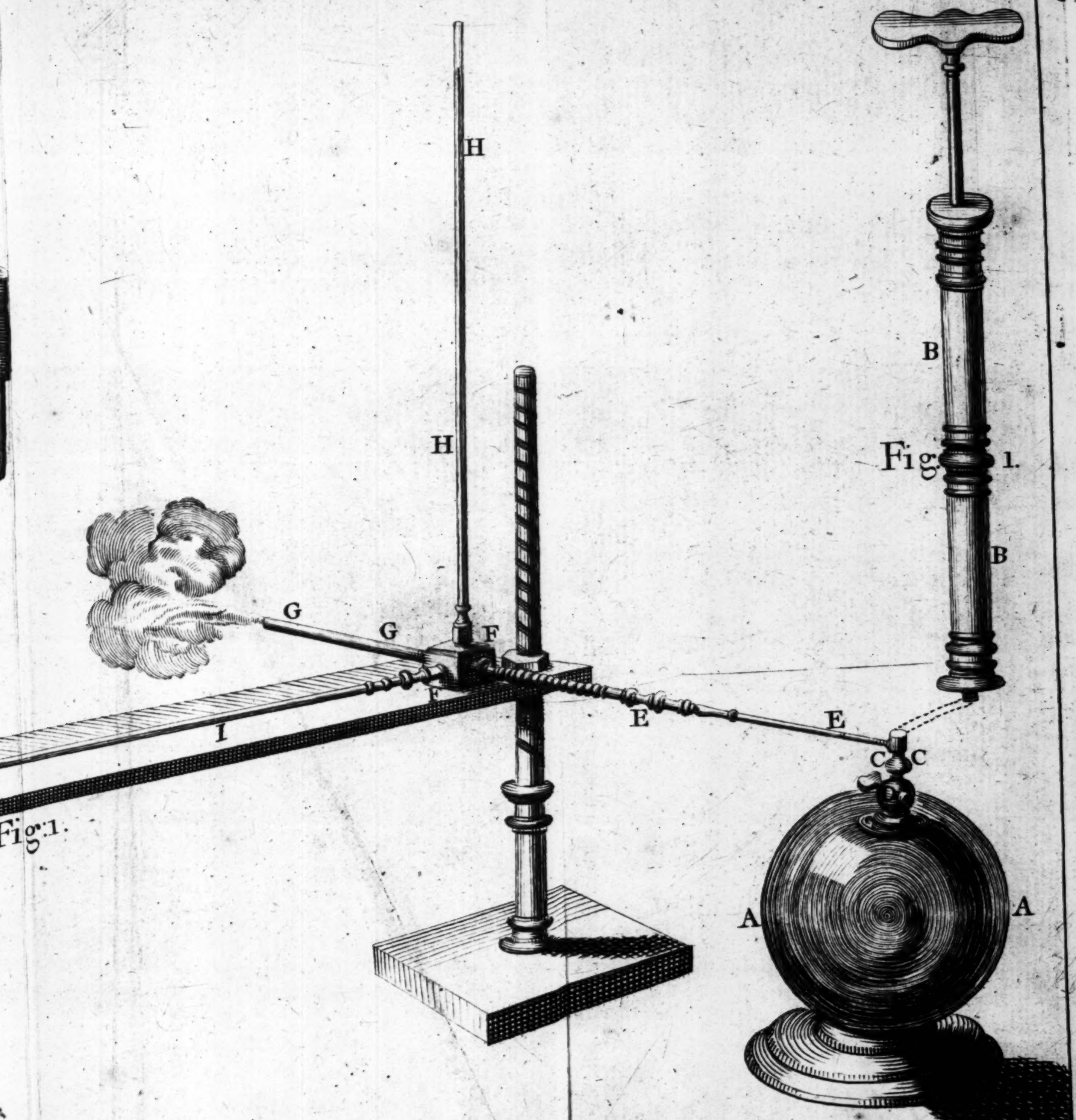

Fig. 1.

alſo. Now there can be no third way; and therefore 'tis univerſally true, that *the moſt rarefied Medium is not neceſſary to the Production of this Phænomenon.*

Experiment V.

THE Experiments hitherto related, have diſcover'd what Mercurial Lights may be produc'd, either in *Vacuo*, or in *Mediums* making ſome Approach thereto. But the following one will ſhew what Light is producible in a Medium very different from either of the former.

In order to this, I took a *Glaſs Tube* (whoſe Content was about 30 Ounces of Water;) and having put into it near half a Pound of pure Quickſilver, I clos'd the Mouth of it with a *Braſs Cap*, which had a *Cock* inſerted in the middle of it, by which means the Mercury had a free Communication with the external Air: This done, the Globe was ſhook, and the Particles of Light preſently appear'd in great Plenty; they were of the Bigneſs of ſmall Pins Heads, very bright and vivid, ſparkling like little diminutive Stars in the *Lactea*, and exhibiting altogether ſuch a whitiſh Appearance as thoſe Stars do. Their Number would encreaſe, according to the Rapidity of the Motion given the Globe: So that by proportioning the Agitation, one might produce a greater or leſs Number of theſe ſmall Luminaries.

See Fig. 5. Plate III.

Having carried the Experiment thus far, I took the ſame Glaſs Globe, with the ſame Mercury included in it, and applied it to the *Pump* (by the help of a hollow *Braſs Pipe*, which ſcrew'd both to the *Cock* and *Pump*;) and then the Air being exhauſted, and the Cock turned to prevent its Ingreſs, the Globe was taken off, and moderately ſhaken. And now the *Phænomenon* was quite alter'd: For the Mercury appear'd luminous all round: It did not now diſcover, as before, a Congeries of little, bright, twinkling Sparks; but a continu'd Circle of Light, which laſted all the time of the Agitation And if that Motion were check'd with another of greater Violence, it would then appear luminous almoſt all over the Globe. This being try'd, the Air was admitted again into the Cavity of the Globe, and then the Mode of Light returned to its former Appearance: The continued Circle of Light was loſt (neither could it be recover'd again by any ſhaking whatſoever) and the little Stars return'd ſparkling as before.

From what has been here related, we may infer, *Firſt*, That *Light is producible by the Agitation of Mercury in the open Air.*

Secondly, That *this Light producible in the open Air, is very different from that produc'd in* Vacuo, *or a much rarefied Medium.*

Thirdly, That *the Difference between theſe Lights conſiſts particularly in this; that the luminous Particles are* **diſtinct** *and* **ſeparate** *in the one, and* **united** *or* **blended** *into one continu'd Body of Light in the other.*

Fourthly, That *the Preſence and Action of the Air is the Cauſe of the Separation of the Parts of the Mercury into ſo many diſtinct luminous Globules, which in* Vacuo *form'd all one continu'd Circle.*

And from hence it appears, that the Accounts given in this Experiment are not at all contradictory to any of the former, about the Mercurial *Phoſphorus*: For *there* I ſpeak of one kind of Light, and *here* of another. That Light which is produced in *Vacuo*, or a very much rarefied Medium is not the ſame with this produc'd in the *Air*: And therefore when I ſay, that the Mercurial *Phoſphorus* is not producible, but in a Medium ſo or ſo qualified, it ought to be obſerv'd, that I ſpeak there concerning a Light of the ſame Kind and Quality with that diſcover'd upon the Agitation of the Mercury in the Barometer: For *that* is vaſtly different from *this* which appears upon the ſhaking of the Mercury in the open Air.

SECT. II.

An Account of ſeveral Experiments made concerning the Attrition of Bodies in various Mediums.

The Deſcription of the Machine *for giving a ſwift Motion to Bodies in* Vacuo, *without admitting the External Air; repreſented by Plate II.*

A Deſcription of the *Air-Pump (Fig.* I.*)* being before given, I ſhall forbear taking any further notice of it, ſaving what immediately relates to the following *Experiments*.

AAAA IS a Ladder, ſuch as is generally us'd in Houſes.

BB Is a Bar of Iron, which paſſes through he middle of the upper Step, and is faſtned o the Back-board of the Ladder by two Nuts nd Screws thro' both the Board and Iron.

CC The Jaws of the Iron Frame which holds the great Wheel DD, of 23 Inches Diameter within its Groove.

EE The Braſs Plate of the Air-Pump, on which the Recipient ff is placed.

gg The Spindle, to which Bodies of different Magnitudes may be faſtned, by a Hole paſſing through the middle of them, ſufficient to receive the Spindle; and by means of the two Nuts hh, a larger or a ſmaller Body may be ſcrew'd faſt on.

ii Is a Braſs Plate turned true to the ground Edge of the Recipient on which it is placed, having a Braſs Box in the middle of it, which is full of Collars of Leather well Oil'd, through which the Spindle paſſes; the Hole of the Braſs being likewiſe juſt fit to receive it.

kkkk Two Pillars, with Nuts to ſcrew down a Piece of Board, which has an Iron faſtned to it to receive the upper Point of the Spindle; the lower one falling into a Braſs Socket, ſcrew'd to the middle of th Plate of the Air Pump.

LLLL The Supporters, reaching from the upper Board of the Ladder to the Pillars to prevent the Recipient's being drawn from its Place by the Motion and Tugg of th Wheel-Band.

m m The ſmall Wheel, which the Band from the great one ſurrounds, and is one Inch and half in Diameter.

N N The Winch which gives Motion to the whole; the ſmall Wheel m m making about fifteen Revolutions to one of the large Wheel D D: So that a Body faſtned to the Spindle g g, of the ſmall Wheel m m, will be turn'd fifteen times round to once of the great Wheel: And according as that ſhall exceed in Diameter the ſmall Wheel, ſo will the Velocity of the Motion of the extream Parts be proportionably encreas'd.

o o o o A ſtrong Board reaching from the lower Jaw of the Machine to the Ground, for the ſupport, or giving a ſteady Motion to the great Wheel.

p p Are two Screws, which faſten the Ladder to the Floor.

Experiment I.

Concerning the Attrition of Amber on Woollen in Vacuo.

I Took ſome *Amber Beads*, of the bigneſs of ſmall Nutmegs; and having paſs'd a Thread through 'em, apply'd 'em by that means to a circular piece of Wood, which was *turn'd* with a *Groove* on the edge of it, on purpoſe to keep the Beads from being diſplac'd by the ſmart Friction they were to endure. Likewiſe, for their better ſecurity and faſtneſs, there were ſo many *Pins*, or pieces of ſmall Wire, driven thro' the Wood; and between every Bead there was a String ty'd over from *Pin* to *Pin*; the Beads, at the ſame time, ſtanding out by the ſpace of their Semidiameters beyond the body of the Wood into which they were fix'd.

In this manner was the whole put upon the *Spindle*, and made faſt there by the two *Nuts*, (as was expreſs'd before in the *Deſcription* of the Machine:) Then the *Braſs Plate*, on which the *Woollen* was wrapt, being ſcrew'd to its place, (by means of the *Socket*, which receives the lower Point of the *Spindle*;) would ſpring back, and graſp the Amber with a moderate force.

See Fig. 2. *Plate* V.

force. Theſe things thus prepar'd, the *Receiver* was plac'd over them all together, with its upper Plate and *Box* for the *Spindle* to paſs thro': The *Pump* then being ſet to work, the Mercury in the Gage was in a very little time elevated to about 29 Inches and a Half; which ſhew'd the Receiver to be well exhauſted. And now the great *Wheel* of the Machine being turn'd, the Amber had a very ſmart Attrition on the Woollen. At firſt nothing remarkable appear'd; but, in the ſpace of a *Second* or two of time, there was *a Light which became ſenſible enough:* For where ever the Attrition of the Amber was made; while the Motion went on, there, and at all times, did the Light continue without intermiſſion, and might be diſcern'd at three or four Foot diſtance. Indeed it would not continue if the Amber did any way deſert the Woollen, notwithſtanding the exceeding Velocity of the motion: But where the Attrition was uninterrupted, the Appearance of the Light was ſo too, and propagated it ſelf to that diſtance I have mention'd. And how ſwift the Motion was, which was given in order to the Production of this *Phænomenon*, may be eſtimated from the Diameters of the ſeveral revolving Parts; for the Diameter of the great Wheel was 23 Inches; that of the ſmall one mov'd by it was One and a Half; that of the Wood and Amber on the ſame Spindle with the ſmall Wheel, was $4\frac{1}{5}$. Now, ſuppoſing the great Wheel to make two Revolutions in a *Second*, I thing it follows that the Velocity of the Extream Parts of the Amber muſt be at the rate of a Mile in three minutes. And this violent Motion (as it may well be expected) was attended with Heat alſo, and ſuch as diſcover'd it ſelf by plain and ſenſible marks; for the Amber (beſides what the Touch diſcover'd) appear'd manifeſtly to be burnt and crack'd; and the Woollen was not only diſcolour'd, but perfectly ſcorch'd, by the intenſe Heat.

I will conclude the Account of this Experiment with this one Remark, relating to the Light produc'd by this Attrition: *viz.* That tho' the ſame Motion and Friction was given the Amber in the *Open Air* as in the *Vacuo*, yet in the former Caſe the Light was *very ſmall and faint*, in Compariſon of what it was in the latter.

By this Experiment therefore we learn,

1. *That Light is producible in* Vacuo, *by the Friction of a ſolid Body (as Amber) againſt a Body of a ſoft and yielding nature, (ſuch as Woollen.)*

2. *That this is not a mere lambent Fire, but ſuch as is accompanied with a great Heat.*

3. *That this Light depends ſo immediately on the Attrition as to diſappear where that ceaſes.*

4. *That it requires a very thin and rare Medium, in order to its Appearance : And the thinner the Medium, the greater the Appearance.*

Experiment II.

Concerning the Attrition of Flint *and* Steel *in* Vacuo.

HAving provided a *Steel* Ring about 4 Inches Diameter, and ⅛ of an Inch thick, I fix'd it between two pieces of Wood (of a leſs Diameter) on the *Spindle* with the *Nuts*, as in the foregoing Experiment ; the Edge of the Ring ſtanding out beyond the Extremity of the Wood which held it, about half an Inch. The *Braſs Plate* (which I made uſe of for faſtening the *Woollen* for the Attrition of the *Amber*) ſerv'd here alſo to fix a Piece of Flint, an Edge or Corner of which was placed towards the Steel: And this Braſs Plate, by vertue of its Spring, would hold the Flint pretty ſtrongly to the Steel, notwithſtanding ſome parts of it might be worn or chipp'd off by the Rapidity of the Motion: And in this manner 'twas cover'd with a *Receiver*, a *Braſs Plate* and *Box* (as the former was.)

Before any Air at all was exhauſted, the great Wheel was turn'd, which gave a motion to the ſmall one, and conſequently to the Steel; and by its Colliſion with the Flint, there were Sparks of Fire produc'd in *great plenty:* But after ſome Air had been withdrawn, and the motion given, as before; the Sparks which then appear'd, were neither ſo *numerous* as before, nor ſo *bright* and *lively*. And as more and more Air was ſtill drawn out of the Receiver, ſo this Change in the Sparks produc'd, became more and more manifeſt. At *every ſtop* made, to repeat the Experiment in an *higher degree of Rarefaction*, I found the Sparks ſtill to diminiſh, both in their *Luſtre* and their *Quantity*; till at laſt, when the Receiver came to be well exhauſted of Air, there *was not one Spark to be ſeen*, tho' a much greater motion was given than before, and conſequently a more ſtrong and valid Colliſion of the Flint and Steel. All the Appearance in this caſe was, only a *faint*, *continued*, *little Streak of Light*, viſible on the edge of the Flint that was rubb'd by the Steel.

This being try'd, there was then ſome Air let into the Receiver, upon which (the motion being given as before) *ſome* Sparks were diſcover'd, but of a *dull gloomy Hue :* Upon the letting in a little more Air, by I know not what Accident, the whole quantity of Air forc'd it ſelf in, and then the Wheel being ſet to Work again, *the Sparks appear'd as numerous and as vivid as at firſt.*

The Concluſion therefore from this Experiment is, *That the Air's Preſence is abſolutely neceſſary to that vigorous expanſive Motion of the Parts of Bodies, wherein the Nature of Culinary Fire conſiſts.*

Qu. Whether the Light viſible on the edge of the Flint, when the Receiver was well exhauſted, was not of the ſame (*lambent*) kind with other Lights produc'd by the Friction of certain Bodies, of which mention is made in ſome of our Experiments?

Experiment III.

Concerning the Attrition of Glaſs, *and various other Bodies, in* Vacuo.

1. *Concerning the Attrition of* Glaſs *and* Woollen.

I Took a *Glaſs Globe* of about Four Inches Diameter, having a Paſſage through the middle of the *Spindle*, which was faſtned to it with *Corks* and *Screws*. The *Woollen* againſt which the Friction was to be made, was the coarſeſt ſort of that which is commonly uſed for *Gartering*, which I choſe purpoſely on the account of its Haſhneſs, as being likely to improve the Phænomenon to a higher degree than the *Cloth-Liſt* I had uſed before. This was wrapt about the Arms of the *Braſs Spring*, and being ſcrew'd down to its place, gently embrac'd the Globe: Then a large *Receiver* was put over all; and the Pump being ſet to work, in a little time the Air was exhauſted out of the Receiver. The great Wheel being then turned, gave fifteen Revolutions to the included Globe, at each of its own: Which ſwift Motion giving a ſmart Attrition on the Woollen, quickly produced a beautiful Phænomenomenon, *viz.* a fine *purple* Light, and *vivid* to that degree, that all the included *Apparatus* was eaſily and diſtinctly diſcernible by the help of it. And thus it continued while the Friction laſted.

Upon the *letting in* a little Air, the *Light* and the *Colour* were both chang'd; the Light impair'd in its *Brightneſs*, and the Colour in its *Tincture*. And as the Air, at ſeveral times, was ſuffer'd to return into the Receiver again, ſo did the Light ſtill become more *pale* and *faint*; tho' even when the Receiver was quite fill'd with Air, ſome *feeble Light would ſtill diſcover it ſelf*, upon the ſame Attrition given as at firſt.

I found, that this Purple Light was *viſible no where*, but *only on the Arms of the Braſs Spring*, where the Glaſs in its motion rubb'd upon the Woollen; and that the Dimenſions of it were about half an Inch in Breadth, and one Inch in Height: And farther, That it did all the while remain ſteddy in its poſition, without any the leaſt Undulation, tho' the motion of the Glaſs was ſo conſiderably ſwift.

Some Varieties occurring in the Experiment laſt mentioned, at Various Trials.

When this Experiment came to be repeated two or three times, with the *ſame* Glaſs; no *Purple Light would appear*, but a *Pale* one only ſucceeded in its room; neither could I recover the Purple with that *ſame Glaſs*, by any Methods whatſoever that I could uſe.

When I took a *freſh* Glaſs; after I had made uſe of it (this way) two or three times, the *Purple was loſt again*, and could not be retriev'd. And if a new one were us'd, the Effect would ſtill be the ſame.

Sometimes, if the Glaſs were taken out after a violent Attrition, *it would be ſo hot*, as *ſenſibly to offend the Hand* that held it: And the Woollen would appear not only *diſcolour'd*, but perfectly *burnt* through.

Sometimes the Light would not be confin'd to thoſe ſtrict Bounds already mention'd, but *would be ſpread quite round the Globe*, and *make an entire continued Circle*, all the time of the motion; notwithſtanding that it touch'd the Woollen in no more parts, than it did in the former Experiment.

Sometimes a perfect diſtinct *Halo* would appear, ſpreading it ſelf quite round the fixed Light. This I attribute to ſome *Particles* of *Water*, inſinuating themſelves by the Spindle, through the Box on the upper Braſs Plate (where Water is always kept, to prevent the Entrance of Air in that place:) For this Water deſcending along the *Spindle*, till it reached ſome part which was of a greater extent, would there, as I conceive, by the Violence of the Motion given, be thrown all about the *Receiver* in ſmall Drops; ſome of which being very likely to fall on the *Woollen*, would there be *heated* to a conſiderable degree by the Attrition of the Glaſs; and being conſequently *evaporated*, would appear there in the Form of a *Halo*, ſurrounding the Light. And what confirms this Solution, is, That having ſince form'd a Contrivance to prevent the ſcattering of the Water, no Appearance of any *Halo* has been obſerv'd. But to proceed,

In this Experiment I have ſhewn the Effects of the Attrition of *Glaſs* on *Ordinary Woollen*; I would now add an Experiment concerning the Attrition of *Glaſs* on *Woollen ſome way prepar'd* or qualify'd beforehand.

I took ſome of the forementioned *Liſt of Cloth*, which had been drench'd in *Spirit of Wine*, and faſtned it to one Arm of the *Braſs Spring*: And ſome of the ſame *Liſt*, which had been ſteep'd in Water impregnated with

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In this Experiment I have ſhewn the Effects of the Attrition of *Glaſs* on *Ordinary Woollen*; I would now add an Experiment concerning the Attrition of *Glaſs* on *Woollen ſome way prepar'd* or qualify'd beforehand.

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Salt-petre, I ty'd to the other Arm of the ſaid Spring: But both Pieces were well dry'd before I made uſe of them.

Then, upon the Attrition, I obſerv'd the Light to break from the agitated Glaſs, in a very odd Form, reſembling that of *Lightning*. This is manifeſtly different from the laſt Phænomenon: For there indeed, we had a *delicate Purple colour'd Light*; but here, a brisk *fulgurating* Light, ſcattering it ſelf about in Flaſhes, and darting with a Force from the Surface of the revolving *Glaſs*.

2. *Concerning the Attrition of* Glaſs *and* Oyſter-ſhells.

Inſtead of the forementioned *Woollen*, I made uſe of two flat *Oyſter-ſhells*, well dry'd: Each Arm of the Braſs had one fixed to it. Upon the uſual Motion given, a Light appear'd, reſembling a *fierce flaming Spark*, juſt upon that very Spot where the Glaſs and the Shells touch'd one another. This Light did not dilate or extend it it ſelf, but kept within the bounds where it firſt appear'd; and it was but a ſmall compaſs that it appear'd in.

3. Con-

3. *Concerning the Attrition of* Oyſter-ſhells *on* Woollen.

The Succeſs of this Trial, was, that it produc'd a Light, but an *obſcure* and *dim* one, and, at beſt like a faint *Halo.*

4. *Concerning the Attrition of* Woollen *on* Woollen.

I took ſome of the *Liſt* formerly mention'd, and bound it about the edge of a *Wooden Wheel,* which I had caus'd to be *turn'd* for that purpoſe. This Wheel (with its *Round* of Woollen) I faſten'd on the *Spindle* ; and ſome of the coarſe *Gartering* was alſo put about the Braſs *Spring.* The Reſult was, that upon the motion given (as uſual,) a *ſmall glimmering* Light appear'd, but ſuch as gave no Proſpect of *being any way improv'd* by the continuance of the motion: The Woollens were not in the *leaſt diſcolour'd,* tho' the Friction was ſometimes more than moderate; neither was there any ſign at all of ſuch an Effect, as hath been ſhewn, to be produc'd by the Attrition of Woollen on other Bodies: The Light (which had been produc'd) *totally diſappear'd,* upon the re-admiſſion of leſs than a quarter part of the Receiver's natural content of Air, tho' the

Attrition made then, was as great as it had been at any time before.

'Tis further to be obſerv'd, that I could never find, that the *different Colours* of *Woollen* contributed any thing to the *different Colours of Light*, exhibited in any of theſe Experiments.

The ſeveral Particulars of Fact which we learn from theſe Experiments, are reducible to the following Heads:

That a Purple Light was produc'd by the Attrition of Glaſs on Woollen (in *Vacuo.*)

That the Purple Light decay'd, both as to its Tincture and Vigour, upon the admiſſion of the Air; and did more and more do ſo, as more Air was let in.

That this Purple Light is loſt, after two or three ſucceſſive Trials with the ſame Glaſs.

That the bounds to which the Purple Light confines it ſelf, are of different and various extents; reaching ſometimes all round the Globe, *and at other times being* only about the place of the Attrition; *but ſtill ſteady, and without Undulation.*

That a briſk Fulgurating Light was produc'd by the Attrition of Glaſs, on Woollen impregnated with ſaline and ſpirituous parts.

That a Red Fiery Light was produc'd upon the Attrition of Glaſs on Oyſter-ſhells, which Light was confin'd within a narrow compaſs, and did not ſpread it ſelf farther about.

That a faint dim Light was produc'd, upon the Attrition of Woollen on Oyſter-ſhells.

That a ſmall glimmering Light was produc'd upon the Attrition of Woollen on Woollen; which was not encreas'd by the continuance of the Motion, and which diſappear'd upon a ſmall admiſſion of Air.

That in the Attrition of Glaſs on Woollen, not only a Light, but alſo a great Heat was produc'd; diſcoverable by the Glaſs, and its Effects on the Woollen too.

And from theſe Obſervations we may make the following Remarks.

Firſt, *That different ſorts of Bodies afford us remarkably-different Lights*, different in *Colour*, and different in *Force* and *Vigour*. This appears from the particular matters of Fact now ſet down.

Secondly, *That the Effects of an Attrition may be various, according to the different preparation and management of the Bodies which are to endure it.* Thus the Woollen, tinctur'd with Salts and Spirits, gave ſuch a Friction, as produc'd quite a new ſort of Light.

Thirdly, *That Bodies which have yielded a particular Light, may be brought by Friction to yield no more of that Light.*

This is plain, from the *ſtrange and ſurprizing loſs of the Purple Colour*, after two or three Trials made with the ſame Glaſs; for that *Purple* could not be recover'd by any Art or Means whatſoever.

A *Pale* Light indeed, was producible from the ſame Glaſs afterwards, but the *Purple* was irretrievably loſt: Therefore the Purple-colour'd luminous Matter *muſt either be quite ſpent*, (that is, *evaporated and carried off)* by the Attrition, or elſe the *Configuration and Texture of the Glaſs underwent ſuch a change by that ſame Action, that it was no longer capable of admitting the Purple Light.*

If the Cauſe were from the *Conſumption of the Matter*, then we may reaſon thus; That *that certain determinate quantity of colour'd luminous Matter, which is lodg'd in ſome Bodies, may, by a Courſe of repeated Attrition, be quite exhauſted and carried away.*

Further; becauſe a Pale Light is producible after the other is gone, we may likewiſe upon the ſame Suppoſition argue thus: *That ſome ſorts of colour'd luminous Matter are more eaſily ſeparable from Bodies than others*; or, *That they require different degrees of Force and Friction, in order to their evaporation and diſcharge.*

And this is highly reaſonable, ſince the Parts of differently colour'd Lights are undoubtedly of *different magnitudes and bulks*; and conſequently, That Force which may be ſufficient to put ſome of 'em into *vibratory expanſive motions*, may not be ſufficient to produce the like Effects upon others.

On the other hand, if the loſs of the Purple was owing to the *alter'd Tone or Texture of the Glaſs*, then the Concluſion will be, *That Bodies may be ſo changed by proper degrees of Friction, as to retain ſome ſort of colour'd luminous-Matter, which they did once emit (as here the* Purple ;*) and to emit another ſort, which they did at firſt retain* ; (as in our Caſe the *Pale* Light.)

Now, in favour of the *latter* of theſe two (and as I take it, the only two poſſible)Cauſes, it may be alledg'd, that the next Experiment will prove to us a *Diminution* of *Light*, or a *Decay of its Force and Colour*, *conſequent upon an Attrition.* But then, as we ſhall ſee, thoſe Bodies are both *hard* and *inflexible*, ſuch as, by rubbing, would wear and alter one another's Texture : And beſides, the Phænomenon there, is only a *diminution or decay of the Strength and Luſtre of a Colour*; a mere *gradual alteration* of the *ſame* Colour, not the production of a *new* and *different* one. On the contrary, in this Phænomenon now before us, one Colour is *loſt*, and a very different one appears in its room : And the Attrition here us'd, was that of a *ſoft* and *yielding* Body againſt a *hard* and *inflexible* one ; ſo that the *wearing* of the Parts, and the deſtruction of their *Spring* or *Tone* conſequent thereupon, is not here ſo eaſily conceivable. However, upon the whole, I ſhall leave it as a

Query Whether the *Loſs* of the Purple Colour be owing to the *Conſumption* or the *Retention* of the Matter?

Experiment IV.

Concerning the Attrition of Glaſs *on* Glaſs.

Firſt, In *Vacuo.*

I Took a *Glaſs Globe* about three Inches in Diameter, which I fix'd to the *Spindle*; and to the two Arms of the *Braſs Spring* were ty'd two *Slips* of *thin Board*, which had Pieces of a *Glaſs Tube* faſtned to them, by the means of ſome ſmall *Neal'd Wires* which were put thro' their Cavities. Theſe Wires likewiſe paſs'd thro' ſome Holes in the Board, made for that purpoſe; and ſo kept the Pieces of Tube firm and tight in their Places. All this was cover'd with a large *Receiver* (as uſual;) and the *Pump* being wrought, the Air was exhauſted.

See Fig. 6. *Pate* III.

The great Wheel being then turn'd, a ſwift Motion was given to the included Globe; and by the Friction of this on the foremention'd Tubes, a *conſiderable Light was produc'd.* The whole included *Apparatus* was perfectly diſcernible by it; and the Illumination had been much greater, had not the Day-light prevented. (For it was then but a little paſt Five P. M. in the Month of *February*, the Horizon being clear, and the Room open wherein the Experiment was made.) The *Colour* of the Light produc'd, reſembl'd that of *melted Glaſs*: And, which is very remarkable, this glowing Colour did not appear only juſt upon the *Parts where the Friction was made*, but alſo at the *Extremities* of the Tubes, where the Globe did not touch them.

Secondly, In *leſs rarefied* and *Common* Air.

THE Air being ſuffer'd to enter the Receiver by degrees, and at ſeveral times, the Motion was given at each of thoſe Ingreſſes of the Air: And the Phænomenon here obſerv'd, was, that *no ſenſible Decay of Light or Colour was to be diſcover'd*, at any of thoſe times: Except only that at laſt, when the Tubes by much rubbing were worn, and conſequently, their Spring leſſen'd and impair'd, then indeed, the Light would be diminiſh'd, in proportion to the Abatement of their force on the moving Body. And this I have often obſerv'd, when the Motion has been made for ſome time in *Vacuo*, or in *Common* Air. So that I queſtion not, but if the Experiment had been begun where it ended, *(in this Trial)* the leſſer Light would then have been exhibited in *Vacuo*. That is, had the Experiment been made, even in *Vacuo*, with Tubes thus rubb'd and worn, and conſequently their Spring weaken'd, a leſs Light would have been produc'd, than if the Experiment had been made, in *Plano*, with Tubes not thus weaken'd and impair'd by the Attrition.

N. B. I have ſince tried this Experiment in *Vacuo*, and that about *Noon*, in a clear Day. And I found, that the Light was even then as ſenſible, as that of a Piece of *Red hot Glaſs* of the ſame Bigneſs would have been in the open Air. Yet this Appearance, as diſcernible as 'tis, *continues not any longer than the Motion is continued.*

By this Experiment we learn,

Firſt, *That a conſiderable Light is producible by the Attrition of Glaſs on Glaſs*; that is, of ſome ſort of hard Bodies on others of the ſame kind, *in Vacuo.*

Secondly, *That this Light continues unalter'd upon the Admiſſion of Air.*

Coroll. *We may ſee the difference therefore between this Light here produced, and thoſe mentioned in other Experiments:* For this endured without Change, the Shock of the returning Air; whereas moſt, or all of thoſe formerly recited, underwent ſeveral gradual Alterations,

ons, according to the Degrees of the Air's Admiſſion.

Thirdly, *That when the Tubes were worn by the Friction, there was an Abatement of the Light: And the more Wearing, the more Abatement.*

Coroll. *We may ſee from hence, how much the due Spring and Tone of the Parts contributes to the Emiſſion of Light, in thoſe Bodies which will emit it.* So that we may reaſonably believe in general, That *very great Alterations may be wrought in Bodies, as to their Luminous Qualities and Properties; either leſſening and deſtroying them on one hand, or perhaps encreaſing and improving them on the other, by the Actions of other contiguous Bodies upon them, according as thoſe Actions tend, either to the weakening or confirming the Spring and Tone of their Parts.*

I have only Two Obſervations to make further, with regard to this Experiment.

Firſt, That the *Glaze* or *Poliſh* on the Surface of a Glaſs, *adds nothing to the Light*, as far as I can find by any Obſervation.

Secondly, That though this Colour be like that of *red-hot* or melted Glaſs; yet the Glaſs is not really in ſuch a State as to be *red hot*; becauſe this *Colour never out lives the Motion*, but is gone preſently upon the ceaſing of it; (as has always been obſerved in the darkeſt Night;) whereas if 'twere really red hot with the Motion, the Appearance muſt *neceſſarily be preſerved for ſome ſmall time*, at leaſt.

Experiment V.

Concerning the Attrition of Glaſs *on* Glaſs, *under* Water.

THIS is no more than a Repetition of the laſt Experiment in another Medium: For the whole Apparatus (there mentioned) was now intirely immers'd in *Water*. Upon the firſt Friction of the Globe on the Tubes, a *pretty brisk Light was produc'd*, which enlighten'd the whole Body of the Water. The *Parts of the Tubes where the Friction was made*, were diſtinguiſhibly *Red*; but the Appearance ſoon began to *leſſen*, and in no long time *quite died away*. For the Water grew thick and turbid, by the *Grit* or *Powder* of the Glaſſes, which was worn off by the *Attrition*. It approached continually nearer and nearer to the Colour of *Whey*; ſo that the Light could be but juſt diſcover'd, as glimmering through the Body of it; and that not *continually* neither, but with *Interruption*, and like *faint Flaſhes* ſent out at a diſtance. Though at ſome other

Trials, I have obſerved the Light to be more conſiderable than it was at this time.

The *Duſt* rubb'd off by the Attrition of the Glaſſes, I view'd through a good *Microſcope*; the Particles of it appeared to be of a long and ſlender Figure; but I could not diſcover the leaſt ſign of their having been any way in *Fuſion* or *melted*.

We ſee therefore, that Light is producible by the Attrition of *Glaſs* on *Glaſs*; not only in *Vacuo*, and *Common Air*, but alſo in *Water* too. And hence 'tis evident further, that the Glaſſes are not heated red-hot by the Attrition; whatever ſuch Reſemblance the Colour carries along with it.

An Experiment concerning the Production of a conſiderable Light, upon a ſlight Attrition of a Glaſs Globe *exhauſted of its Air.*

I Took a Glaſs Globe of about nine Inches Diameter, and exhauſted the Air out of it; then, having turned a *Cock*, which prevented the Return of the Air, I took it from the *Pump*. The Globe being thus ſecured, I fixed it to a Machine, which gave it a ſwift Motion with its Axis perpendicular to the Horizon: And then applying my naked Hand (expanded)

ded) to the Surface of it, the Reſult was, That in a very little time a conſiderable Light was produced.

And as I moved my Hand from one Place to another (that the moiſt Effluvia, which very readily condenſe on the Glaſs, might, as near as I could, be thrown off from every part of it) by this means the Light improv'd; and ſo continu'd to increaſe, till Words in Capital Letters became legible by it, as has been obſerved by Spectators.

Nay, I have found the Light produced to be ſo great, that a large Print might without much difficulty be read by it: And at the ſame time, the Room, which was large and wide, became ſenſibly enlightned, and the Wall was viſible at the remoteſt diſtance, which was at leaſt ten Foot.

The Light was of a curious purple Colour, and was produced by a very ſlender Touch of the Hand; the Globe at the ſame time being ſcarce ſenſibly warm: Neither could I ever find, that a more violent Attrition did contribute any thing to the Increaſe of the Light.

Now after this Attrition of the exhauſted Globe had been continued for ſome time, the Cock was turn'd, which gave liberty to the Air to enter into the Globe through the Joynts of the Screws; the motion of the great Wheel, and the application of the Hand, continuing all the while as before. Then I obſerv'd, that as the Cavity of the Globe became more and more repleniſh'd with Air, ſo the mode of the Light continued to alter, till the ſame quantity of Air was re-admitted, as had been exhauſted. And when ſuch a quantity was once enter'd, there was then as great a difference between that Light and the Light produc'd when the Globe was empty of Air, as between the Lights produc'd from Mercury, when the Experiment was made in *Vacuo* and the open Air. For if a Man touch'd the Globe with his Fingers, there were ſpecks of Light (tho' without any great Luſtre) ſeen to adhere to them. Nay, while my Hand continued upon the Glaſs, (the Glaſs being in motion,) if any Perſon approach'd his Fingers towards any part of it in the ſame Horizontal Plane with my Hand, a Light would be ſeen to ſtick to 'em, at the diſtance of an Inch or thereabouts, without their touching the Glaſs at all; as was confirm'd by ſeveral then preſent. And it was obſerv'd alſo, that my Neckcloth, at the ſame time, at an Inch or two diſtance from the Globe, appear'd of a fiery Colour, without any Communication of Light from the Globe.

This was the Event of this Experiment, at the *firſt time* of making it; in which caſe the Air was let into the exhauſted Globe *all at once*.

But at the *ſecond time* of making this Experiment the Air was not all let in at once (as before,) but *gradually*, and *at ſeveral times*; by which means the Mode of Light produc'd in the ſeveral different Mediums was the better obſerveable: Tho' it muſt be own'd, that here was no great alteration of the Light, either as to its Vigour or Colour, till ſo conſiderable a quantity of Air was let in, as amounted to more than one quarter part of the Globe's natural Content. But many times before half its Content (as near as I could gueſs) was let in, the Light began to branch it ſelf into pleaſant Figures, from that ſide of the Globe touch'd by the Hand; ſo that the whole body of the Globe was fill'd with theſe fine Appearances. And as more Air was admitted, ſo the Stems of theſe Branches of Light became more and more ſlender and minute, ſtriking then alſo againſt the oppoſite ſide of the Glaſs, and rebounding from thence in a manner very ſurprizing and delightful to behold; till at length, more Air ſtill being let in, the Light and the Figures both diminiſh'd, and continued ſo to do, till the Appearance became the ſame as was related at the concluſion of the firſt Trial.

I would only note here, That what difference ſoever there was, as to the latter parts of theſe two Trials, the former parts of both were alike; except only, that in the 2*d* Trial, upon the application of a piece of white Sheeps-leather, a good Light was produc'd while 'twas held to the Globe with the Wooll-ſide next it; but when twas turn'd with the other ſide to the Globe, tho' it was continued thus expos'd for ſome time, yet no Light appear'd: But then turn it again, and the ſame Light would appear as at firſt. And thus it happen'd upon ſeveral times repeating the ſame.

Having taken notice (in the Experiment now recited) *that the higheſt degree of Rarefaction of the Air in the Globe, is by no means neceſſary to the Production of this Light, ſince it would be very little leſſen'd, either as to Vigour or Colour, till (perhaps) more than one fourth part of the Air was let in:* I would add further, that I have often obſerv'd the ſame thing, as to the Light produc'd in the *Mercurial* Experiments; tho' the Colour indeed was not the ſame, for in thoſe Experiments it was always pale. And ſince in thoſe Experiments with Mercury, there is ſuch a ſeeming congruity of Appearances, in all Circumſtances, with thoſe made on the Attrition of Glaſs without it; one might conjecture with ſome probability, that the Light produc'd proceeds from ſome Quality in the Glaſs, upon ſuch a Friction or Motion given it,) and not from the Mercury upon any other Account, than only as it is a proper Body, which, by beating or rubbing on the Glaſs produces the Light. And that which ſeems to back ſuch a Conjecture, is, that having rubb'd the upper or empty part of a Mercurial Barometer between my Fingers, a Light follow'd upon it without any motion of the Quickſilver. But notwithſtanding all this, the Matter is doubtful; and there may (for all that we know) be a Luminous Quality in Mercury, as well as in Glaſs or other Bodies; which the following Experiment, purpoſely made, ſeems to Countenance.

I took a ſmall quantity of Quickſilver, and put it into a Galley pot, wherein Varniſh had often been us'd, by which means the Pot was pretty well lined with it. The Weather at that time was *moiſt*, which influenc'd the Varniſh ſo far as to ſoften it a little. Now when this Galley-pot, with its contain'd Mercury, came to be in *Vacuo*; as ſoon as the Pump was ſhook, a Light appear'd; and this without any Concurrence of Glaſs, or the Aſſiſtance of a more proper and favourable Seaſon. I purpoſely mention the moiſture of the Weather, becauſe a humid Air would ſometimes render the Experiment unſucceſsful, even in Glaſs, or at leaſt take off very much from the Appearance of it.

Farther; I am inform'd by ſeveral Perſons of Credit, That *Mercurius dulcis*, if broken in the Dark, will yield notable Flaſhes of Light. But ſince the Mercury in that Preparation is pointed with Salts, (and each Globule of it conſequently wrapt up in the ſame,) a Man can't be ſure that the Salts do not contribute to the Phænomenon: For I have often obſerv'd, that Loaf-Sugar, when ſtruck or broken in the dark, affords a Light; and I can't tell but Salts, as cloſely united in their parts as Sugar, may upon a violent ſeparation of 'em, do ſo likewiſe: But this I intend to enquire into by ſome farther Trials, *viz.* *Firſt*, I deſign to try whether *Mercurius dulcis* will afford any Light, when broken in *Vacuo*; ſince, if it proceeds from the Mercury, and there be ſuch a Quality in that Body, 'tis highly reaſonable to expect it then, ſo rare a Medium being the moſt proper to diſcover it in. *Secondly*, What the Salts will do without Mercury, both in the open Air and in *Vacuo*; for there are ſome Bodies, which appear luminous in the open Air, and yet totally loſe that Quality in *Vacuo*. As for inſtance; I took a piece of Wood, (which I ſuppoſe, had lain under Ground a conſiderable time,) very moiſt, but not rotten. In the dark it appear'd very vividly of the colour of Fire; but having inclos'd it in a Receiver on the Pump, I found that as the Air was withdrawn, the Fire-like Appearance proportionally decay'd; till at laſt in *Vacuo*, it became perfect void of Light; and then, as the Air was let in again, ſo it recover'd its firſt brightneſs. This I repeated ſeveral times with the like ſucceſs.

But to return now to the Experiment. The matters of Fact to be obſerv'd from thence, are reducible to the *following* Heads.

The *Production of a Light* by the *Friction* of the Hand on the Surface of *the Exhauſted* Globe.

The *great Improvement and Encreaſe of this Light*, by the *Motion* of the *Hand* from one Place to another.

The *Slightneſs of the Friction* requiſite to produce this Light; and its not encreaſing by a more *violent* one.

The *Alteration of the Light*, upon the *Re-admiſſion* of the Air.

The *Continuance of that Alteration*, as more and more Air *was admitted.*

The *Light's communicating it ſelf to Bodies placed near it*, when the *whole Quantity of Air* drawn out was re-admitted.

This, when the Air was let in *all at once.*

But when the Air was let in *gradually*; then *no great Change of the Light*, as to *Colour or Vigour*, till more than $\frac{1}{4}$ of the Globe's natural Content of Air was admitted.

The wonderful *Figures*, *Branchings*, and *Reverberations* of the Light, as more Air was let in, till it came to the Bounds of a certain Quantity of Air admitted, where the Appearance was at the height.

The *gradual Diminution both of the Light and Figures* (after it was come to that Limit) upon the Admiſſion of more and more Air.

An Experiment concerning the Electricity *of* Glaſs, *diſcovering it ſelf in an extraordinary manner, upon a ſmart Attrition of it.*

HAving procured a Tube, or hollow Cylinder, of fine *Flint* Glaſs, about one Inch diameter, and thirty in length; I rubb'd it pretty vigorouſly with *Paper* in my Hand, till it had *acquired ſome Degree of Heat.* I then held it towards ſome Pieces of *Leaf-Braſs*; which were no ſooner within the ſphere of Activity of the *Effluvia* emitted by the Tube, but they began to put into brisk Motions, and yielded the following ſurprizing Appearances.

They would *leap towards the Tube, at a very conſiderable diſtance from it*; nay, I have found, that ſometimes the Diſtance of twelve or more Inches, has not prevented their doing ſo.

Sometimes they would *adhere and faſten* to the Tube, ſettling themſelves on its Surface, and there remain quiet: And ſometimes they *would be thrown off from it with a very* *great force*, even to the diſtance of ſix or ſeven Inches. And not only when they *adher'd to the Surface* of the Tube, would they thus ſuddenly and precipitantly be driven from it; but alſo *in their Motion of Aſcent towards it*, even when they were advanced ſo far as to touch the Tube, this repellent Force would take place, and hurry them *downwards* with a great Velocity.

And (which ſtill adds to the Wonderfulneſs of the Phænomenon) they would often *repeat this alternate Riſing and Falling*; the *Attractive* and *Repulſive* Forces (whatever they are) exerting themſelves as it were by turns; the one drawing up, and the other beating down theſe light Bodies; and that for ſeveral times one after the other.

Neither is this all the Variety which the Phænomenon afforded: For ſometimes they would *move but ſlowly* towards the Tube, ſometimes they would *remain a ſmall time ſuſpended between the Tube and the Table* on which they were firſt laid; and at other times (which is no leſs ſtrange than the former) they would *ſeem to ſlide along in the Direction of the Sides of the Tube*, and that *without touching it.*

But beſides theſe, there are yet ſome other Obſervations relating to this Experiment, which I think not amiſs here to mention.

Firſt, The hotter the Tube was made by rubbing, to the greater diſtance did the attractive Force extend it ſelf. But that it will anſwer in proportion to any Degree of Heat excited, is what I will not venture to determine. I mean, whether encreaſing ſtill the Degree of Heat in the Tube, the Sphere of the attractive Power will ſtill be proportionably enlarged; or whether there may not be ſome certain Degree of Heat, which may carry the Attraction to its utmoſt Limits of Diſtance, ſo that all Degrees above that, ſhall produce leſs powerful Effects this way: Whether this be ſo, or no, I ſay, 'is a Point which will require farther Trials, and more Conſideration, in order to a compleat Deciſion of it.

Secondly, Tho' all the various Appearances now mentioned, as to the Motions of the attracted little Bodies, do not happen at every Trial, yet many of them are obſervable at all times; neither are there any hinted, but what I have at ſome time or other, punctually obſerved.

And the reaſon of this difference ſeems very probably to ariſe from the different Temper and Conſtitution of the Air. For when the Air is clogg'd, either with *humid and aqueous*, or other *more groſs and ſolid Parts*, raiſed up from the vaſt Fund of Terreſtrial Matter here below, there's no doubt but the Reſiſtance theſe fine *Effluvia* then meet with in their Way, muſt be much greater than when the Air is free, and no ſuch Impediments lie to oppoſe them in their Paſſage. For the *Effluvia*, how ſubtil ſoever they can be imagin'd to be, are yet *Body* and *Matter*, and muſt therefore be liable to the common Laws of Bodies, which is to be reſiſted in ſome proportion to the Strength and Denſity of the *Medium*. Neither is it improbable, but that in a moiſt Conſtitution of Air, the watry Parts may run together, and condenſe on the Surface of the Tube, and ſo choak up, and obſtruct the Paſſage of the Matter, which otherwiſe would be emitted very vigorouſly from thence.

And indeed, I have always obſerv'd, that Moiſture is a great Enemy to all Experiments of this kind: The reaſon of which, I think, is pretty obvious from what was juſt now hinted about the *Reſiſtance*.

However, there is an Experiment, which looks, as it were, like an ocular Demonſtration of this; in which, the Reſiſtance of the *Effluvia* by certain little Bodies interpos'd, was render'd manifeſt even to the Senſe. For having placed a Piece of fine *Muſlin* between the Tube and the forementioned Pieces of Leaf Braſs, all the Excitation I could give the *Effluvia* by ſtrong rubbing, would not raiſe them ſo far, as to make them give the leaſt ſenſible Motion to any one of thoſe little Bodies, even tho' the Tube was held very near them:

them : Yet at the ſame time, if the Muſlin were taken out of the way, the Effluvia would impreſs thoſe Bodies ſo vigorouſly, as to give them a Motion at 3 or 4 times that diſtance. And this Phænomenon, I think, will eaſily be allow'd to be the mere Effect of the Obſtruction of the Paſſage of the Effluvia by the fine Threads of the Linnen ; which Threads either wholly ſtop'd and detain'd, or elſe broke and obtunded the Force of thoſe Active Parts emitted from the Tube by Friction ; ſo that they could not ſhake or agitate the light Bodies, that lay expos'd to them.

I could add moreover, That not only the *moiſt*, but the *cold* Temper of the Air, may be partly a cauſe of the Differences diſcernible in the Effects of this Experiment : For when it was firſt made it was Summer-time as well as dry Weather, and the ſucceſs ſeem'd to be more conſiderable then, than it has been ſince the declenſion of that Seaſon of the Year. And indeed, conſidering all thoſe Effects of the Effluvia upon other Bodies, muſt depend either upon the *quantity of the ſubtile Matter emitted*, or, *the force and velocity of its impulſe*, or elſe upon *both theſe conjunctly* ; 'tis not ſtrange that warm Summer-weather ſhould cauſe a conſiderable difference : Becauſe the vigorous Action of the Solar Rays does then more effectually ſhake the Parts of all Bodies, opens and unlocks their Pores, and ſo makes way for a freer and more plentiful Emiſſion of their Effluvia ; and becauſe alſo, at the ſame time, the more fine and rarefied ſtate of the ambient Medium (the natural reſult of a predominant Heat) will allow them to expand themſelves with more Advantage. However, in colder Weather, the Effects are very ſenſible, and great enough (notwithſtanding the Diſadvantage ariſing from thence) to procure any curious Perſon's Attention to the Phænomenon.

Thirdly, When the Tube became hotteſt, by the ſtrongeſt Attrition, the Force of the Effluvia was render'd manifeſt to another *Senſe* too, namely, that of *feeling*. They did not then only produce all the forementioned Effects in a more remarkable manner, but were alſo plainly to be *felt* upon the Face, or any other tender part, if the rubb'd Tube was held near it. And they ſeem'd *to make very nearly ſuch ſort of ſtroaks upon the Skin, as a number of fine limber Hairs puſhing againſt it* might be ſuppos'd to do. This vigorous Action of the Effluvia put me upon an Attempt, to find in what manner ſuch a Motion was propagated, and in what Figure or ſort of Track it went along. For which end, I held the rubb'd Tube near the *Flame* of a Candle, *Smoke*, *Steam*, *Duſt*, and the *ſurfaces of Liquids* ; but without any manner of ſucceſs.

The reaſon of which, I attribute to the impediments the Effluvia met with from theſe Bodies the Tube was plac'd near. For the ſmall parts of Duſts and Powders, the ſteams of Liquids, the oleaginous Fumes of Flame, and the like ſort of Parts in Smoke it ſelf, immediately adher'd to the ſurface of the Tube, and ſo kept in the Effluvia: Which therefore requir'd the Aſſiſtance of a freſh Attrition to open their paſſage and give them vent again.

And thus much for the firſt part of this Experiment; in which *the Tube was rubb'd while it continued full of Air.*

Secondly, We are now *to conſider the Effects of the Attrition of the ſame Tube, when the Air was exhauſted and drawn out of it.* And here I obſerv'd, that tho' it were rubb'd with equal or greater Force than was employ'd about it when full of Air, *yet the attractive Power was very little diſcernible.* The Effluvia ſcarce diſcover'd themſelves, by any Motion or diſturbance given to the Leaf-Braſs, even tho' it was plac'd within a quarter of the diſtance at which it had been attracted before. Indeed, when the Tube was very warm, and held very near, there would be ſome little Motion given to thoſe ſmall Bodies; but 'twas too ſmall by far to be compar'd with what was impreſs'd when the Tube was full of Air. Beſides, I doubt not but there was ſome portion of Air left in the Tube, and ſo the Attraction might continue in proportion to the Quantity of Air remaining: Or, perhaps, the Heat produc'd by a ſmart Attrition of the Glaſs, may (in this Experiment, as well as ſome others) be in the ſtead, and ſupply the Place, of ſuch a Quantity of that Element.

But to return to the Experiment. When the Air was let into the Tube again, the attractive Power was immediately reſtor'd. Before any new Attrition could be given the Tube, or that it was remov'd from the diſtance and poſition it was held at when exhauſted, even then did ſeveral of the forementioned Bodies (which before ſeemed to be wholly at reſt) begin ſuddenly to move; and ſome of them were attracted up to the Tube it ſelf. And then, laſtly, upon a freſh Attrition, the Tube recover'd its *Electrical* Quality as vigorouſly as at firſt,

Hitherto we have conſider'd the Phenomena of the Tube both when full of Air, and when exhauſted; but in each caſe *ſtill in the Light.* are now to conſider what occurr'd upon the *like Attrition given it in the Dark.* And firſt, the Tube being *full of Air*; 'tis obſervable, that when the Glaſs became warm, *a Light would continually follow the Motion of the Hand* backward and forward: And at the ſame time, if another Hand was held near the Tube, a Light would evidently break forth from it, and That *accompanied with a Noiſe*, reſembling that of the cracking of a green Leaf in the Fire; tho' not ſo loud: Yet when the Experiment has been made in a very ſtill and ſilent Place, I have heard ſeveral cracks at ſeven or eight Foot diſtance, or more. Farther; if any other Object (beſides the Hand) was brought near the Tube, tho' it did not touch it, yet *a Light would fix upon it*, and give much the ſame Appearance as upon the Hand; as I have tried with *Gold*, *Silver*, *Braſs*, *Ivory*, *Wood*, &c.

But now, when the Tube was *exhauſted of its Air*, there was a conſiderable difference, as to this Light and its Effects: For upon the firſt Attrition of the Tube, *a much greater Light indeed did enſue*; but then the *Quality of giving Light to a Body held near it*, *ſeem'd to be quite loſt*: And (which is another Difference no leſs remarkable,) the Light produc'd upon the Attrition of the exhauſted Tube, appear'd to be wholly within it; whereas that which was diſcover'd when the Tube was full of Air, ſeem'd to be altogether on its Outſide.

And thus much for the Experiment, as made upon *a Tube or hollow Cylinder of Glaſs*.

Having now, in the next Place, procur'd *a ſolid Cylinder of Glaſs*; I made the ſame Trials with that as the former; and *found no great difference in the Effects*; only the Emiſſion of the Effluvia *ſeem'd to continue a little longer*, *but not to attract at a greater diſtance* *than the other* (as far as I can yet diſcover.) With this new ſolid Tube, I made the following Experiment.

I took a little Quantity of *Lamp Black*, and having dried it on a Paper before the Fire, I expoſed it to the Tube (which had been rubbed till it was warm :) And it was no ſmall Entertainment, to ſee how briskly the little black Particles were agitated by the force of the Effluvia from the Glaſs. They appear'd to riſe and fall, to move upwards and downwards with great Velocity. And though their ſpecifick Gravity was ſo ſmall, that when they fell by their own Weight, they could not be heard ; yet they were return'd upon the Paper with ſuch a force from the Tube, that their Strokes upon the ſame made a very ſenſible Noiſe. So great was the repellent Force of the Effluvia from the ſolid Tube.

So that now laying the Accounts of this Experiment together, we have the following Heads of Matter of Fact to conſider.

The *various ſurprizing Motions of the Leaf-Braſs*, expos'd to the hollow Tube, upon the Attrition of it.

The *Encreaſe of that Effect*, upon the Encreaſe of the Heat in the rubb'd Tube.

The *Difference obſervable in that Effect*, according to the different Temper and Conſtitution of the Air.

The *Deſtruction or Ceaſing of that Effect*, upon the Exhauſtion of the Air out of the Cavity of the Tube.

The *Return of that Effect in a good degree*, upon the Return of Air, even without the help of any new Attrition; and the compleat Recovery of it to all degrees, when that Attrition was again given.

The *Emiſſion of a Light* from the Tube full of Air, when rubb'd to a degree of warmth in the dark: And this Light accompanied with a cracking Noiſe.

That Light's fixing and ſettling it ſelf upon Bodies laid in its way.

The *Loſs of this Quality* (of fixing upon other Bodies) though with the Advantage of a bigger Light; when the Attrition was given to a Tube exhauſted of its Air.

The *different Seat and Place* of the Light, when the Tube was full of Air, and when exhauſted: Being in the *former* Caſe *without*, and in the *latter*, wholly *within* the Tube.

Laſtly, The ſame Effects, except what depended on the Exhauſtion, produc'd by a *ſolid*, as by a *hollow* Tube.

Having now deduced ſome things from the Experiment, which may, I think, paſs without much Diſpute, for Truths and Matters of Fact; I would propoſe ſome things by way of *Query*, in order to a farther clearing of this Subject.

Query 1. How is it that Attrition does excite and bring forth the Effluvia of Electrical Bodies? They are, in the preſent caſe, propagated from the Body of the Glaſs with a conſiderable force: What is the immediate cauſe of that *Impetus*? And by what means do they come to be ſo affected?

Query 2. Why are the Effects of the Effluvia ſo much greater when the hollow Tube *is full of Air*, than when it is *exhauſted*?

Query 3. Why does the Light produced upon the Attrition of the *exhauſted* Tube, appear wholly *within* it; and That produced upon the Attrition of the Tube *full of Air*, appear altogether on its *Outſide*?

Query 4. Can the preſerving, or the taking away the Equilibrium between the external Air and the Air included in the Cavity of the Glaſs Tube, be of any moment towards the Production of the Effects mentioned in the two laſt *Queries*?

A Continuation of the Experiments on the Attrition of Glaſs.

I Procur'd a Glaſs of a Figure as nearly *Cylindrical* as might be, whoſe Diameter and Length were each about ſeven Inches. The Axis of this Glaſs lying parallel to the Horizon, and the contained Air being exhauſted, it had a Motion given it by a Machine of a new Contrivance. And the Effects of this, with reſpect to the Light produced upon the Attrition of it, were much the ſame as thoſe in the Experiments formerly mentioned. But when the Air was let in again, and the Motion and Attrition given as at firſt; I was ſurprized with the Appearance of a *briſk and vigorous Light continued between* the Point *of my Finger and the Glaſs.* It was not only plainly viſible on the Finger; but beſides, ſeem'd, as it were, to ſtrike with ſome force upon it, being eaſily to *be felt by a kind of gentle Preſſure,* tho' the moving Body was not touch'd with it by near half an Inch.

Plate VII

This Light ſeem'd to iſſue from the Glaſs with *a conſiderable Noiſe,* not much like that of Wheezing, tho' ſomething ſmarter; and it was eaſie enough to diſtinguiſh it from the Noiſe made by the working of the Engine, which notwithſtanding was not a ſmall one. And the Phænomenon was the ſame, as to both the Parts of it, (I mean, both the *Light* and the *Noiſe*) when the Experiment was made in the Day light, as when in the Dark.

For in a very light Room, an Hour or two after Noon, the Glaſs being put into motion, and the Attrition made, and the Finger approached near it, as before; a pure Purple Light immediately extended it ſelf from the Finger to the Cylinder, and was accompanied with the like Noiſe as before mentioned.

And this Trial I have repeated ſeveral times, at different Hours; but ſtill with the ſame ſucceſs.

I render'd theſe luminous Effluvia more remarkably conſpicuous, and at the ſame time more pleaſing to the Eye of a Spectator, by another Experiment made after this manner:

I took a Bit of Fine *Muſlin*, and ſew'd it to a couple of Wires bent circular-wiſe, that ſo it might ſurround the upper Surface of the Glaſs, which it did at near four Inches diſtance: The Muſlin I made as ragged as I could, by breaking the Threads of it every where.

The Glaſs being then put in motion, and the Friction made, as uſually; the Light threw it ſelf abroad vigorouſly, and ſettled in ſmall lucid Sparks upon the Ends of the torn Threads;

looking there like ſo many little Stars, ſeen by a good Telleſcope in the *Milky Way.*

And from all theſe little Balls of Light together, there reſulted ſuch a ſort of a Whiteneſs, as may be obſerv'd in the Heavens by the faint and weak Lights of all thoſe ſmall Stars mingled together.

In all theſe Caſes I could never find *that the Addition of any External Heat would do any thing to encreaſe the Appearance of the Light produc'd.* I tried by placing a red hot Iron juſt under the moving Glaſs; but this would do nothing at all, without the Attrition of the Glaſs; and if the Glaſs were rubb'd, the Effect was no greater, than if the hot Iron had not been there. And the reſult (as to this matter of *heat,)* was the ſame, both when the Glaſs was exhauſted of its Air, and when it was full.

And thus much, as to the Light *produc'd by the Attrition of the Cylindrical Glaſs.*

Secondly, As to the *Electricity* of it; I did not find this Effect more conſiderable here as to Quantity than what was related in the former Experiments of the Tubes. But notwithſtanding, I have diſcover'd ſome Properties of this Electrical Matter, which may ſeem wonderful to thoſe who nicely conſider them; *ſince they afford us a ſort of Repreſentation of the great Phænomena of the Univerſe.* For, having obſerv'd that light Bodies, plac'd near any part of the rubb'd Cylinder, ſeem'd to be equally attracted; I contriv'd a Semicircle of Wire, which I could faſten at a conſtant diſtance, making it encompaſs the upper Semicylindrical Surface of the Glaſs, at 4 or 5 Inches diſtance. This Wire had ſeveral pieces of Woollen Thread faſten'd to it, ſo as to hang down from it at pretty nearly equal diſtances. The length of them was ſuch, that being extended in a Direction towards the Center of that imaginary Circle on the ſurface of the Glaſs, in the Plane of which the Wire was plac'd; they would then reach within leſs than an Inch of the Circumference of that Circle: But if left to their own Liberty, they hung in that parallel poſition to each other, which is repreſented in *Fig.* 1. *Plate* VII. The Cylinder was plac'd with its Axis parallel to the Horizon; and in this poſture, it was turn'd ſwiftly about; and then by the rapid Motion and Agitation of the ſurrounding Air, the Threads were forc'd into ſuch poſitions, as are expreſs'd in *Fig.* 2. *viz.* they were lifted up and bent upwards from the Axis of the Cylinder.

All this while, here was only the ſwift Motion of the Cylinder round its Axis, without any Attrition. But now, when I came to apply my Hand to the lower part of this Glaſs (ſo ſwiftly whirl'd about) and conſequently to add *Attrition* to the former Motion; *the*

Threads preſently began to change their direction, and all harmoniouſly pointed Fig. 3. *to the Center of the Circle, in whoſe Plane the Wire was plac'd.* Neither were they at all diſorder'd or flung out of that Poſition, by the Wind occaſion'd by that violent Motion; but (as if there had been no ſuch hurry of the Air about them) they ſtill perſiſted in their central direction. And to render it moſt ſenſibly convincing, how abſolutely this Effect depended upon the Attrition; I found *I could by ſhifting the Place of the Attrition hither or thither, draw the Threads towards this or that end of the Cylinder; but yet they all ſtill went uniformly converging towards ſome Center in the Axis of it; ſo that they form'd themſelves into a ſort of Conical Surface.*

Farther; if the Wire with its looſe Threads was revers'd, ſo as to encompaſs the lower part of the Cylinder, (as before it did the upper part;) yet the Effect ſtill anſwer'd with the ſame exactneſs. *For the Threads were all erected into ſo many ſtrait* Fig. 4. *lines, ſtill directing themſelves to a Center in the Axis of the Glaſs.*

Hitherto the Axis of the Cylinder was plac'd *Horizontally*; in the next place, I ſet it in a *virtical* poſition, ſo that it ſtood perpendicular to the Plane of the Horizon, in which caſe I made uſe of a Wire-Hoop, which was neceſſarily to be plac'd parallel to the Horizon, that it might encompaſs the Cylinder in the ſame manner as the Semicircular-Wire did before: Only, one ſmall part of this Circular Wire was left open, to make way for the touch of the Hand, which was to give the Attrition. And the Wire being thus plac'd, it was evident that the Threads (without ſome external Force to ſupport 'em) muſt all flagg and hang perpendicularly downwards. Yet, as ſoon as the Motion and Attrition were given, the Threads preſently began to be extended; and, as if they were become ſtiff and hard, form'd themſelves into an Horizontal Plane; their looſe Ends pointing to a center in the Axis of the Glaſs, (as before.)

And thus in all ſorts of Poſitions whatſoever, both of the Wire, and of the Glaſs too; were the Threads acted by a ſort of centripetal Force, to the Laws of which they were always conformable.

The matters of Fact obſervable in this Experiment, may be compris'd under theſe Heads.

The *Continuation* of a brisk Light between the Hand and the Glaſs.

The *Senſible Force* and *Preſſure* of that Luminous Matter; which was very eaſie to be felt.

The *Noiſe* accompanying the Eruption of the Luminous Matter, and the Loudneſs of it, which render'd it diſtinguiſhible from that of the Engine.

The *Conſtancy* of the Light and Noiſe at all Seaſons.

The *Indifferent Regard* of this Light to the Preſence or Abſence of Heat.

This as to the *Light*.

As to the *Electricity*.

An *Equable Diffuſion of Electrical Matter*, in Planes parallel to the Baſis of the Cylinder; and the *Direction* of the Threads to the Center of thoſe Circular Planes.

The *ſteddy and unvaried* Direction of the Threads, notwithſtanding the Wind occaſion'd by the Violence of the Motion.

The *Eaſie Excitation* of the Electrical Matter in any of the parallel Planes of the Cylinder, occaſion'd by the ſhifting of the Hand into a new Place: Alſo the *Variation* of the *Figure*, and *Direction* of the Threads, conſequent thereupon.

The *Conſtant Direction* of the Threads to ſome Center in the Axis, in *all the Poſitions* of the Wire, and of the Cylindrical Glaſs; the Effect being ſtill the ſame, whether the Wire was *above* or *under*, or the Glaſs placed with its Axis either *parallel* or *perpendicular* to the Horizon.

Some further Experiments relating to the Electricity *of* Glaſs.

IT has been ſhewn before, that the Ends of the Threads which were diſpoſed in the the *Semicircular Wire*, were, upon the Attrition, directed to a *Center* in the *Axis of the Cylindrick Glaſs.*

There are ſome other Obſervations and Diſcoveries which I have ſince made, relating to the ſame thing; which are no leſs ſurprizing; and which I ſhall therefore here give in their Order.

Experiment I.

WHEN the Attrition of the Glaſs has been continued a little while, and the *Woollen Threads* laid hold of by the *Effluvia*; then, tho' the Glaſs had no Motion at all, and no Friction continu'd, yet would all the *Threads* continue in their *ſtrait directed Poſture*, and that for the ſpace of four or five Minutes: Nay, it would ſometimes be longer before they could diſengage themſelves from the Force and Action of the *Effluvia*.

And while the *Effluvia* acted with ſo much Vigour, as to ſuſtain the Threads in their *Central Direction*; if a Finger, or any other Body, were put near the Extremities, or *Pointing Ends* of the Threads, they would *avoid and fly* from it, juſt as a *Magnetick Needle* does, when the *diſagreeing* Pole of the Loadſtone approaches to it.

But if the Finger were held at about an Inch diſtance from the End of any ſuch Thread, the Thread would (uſually, tho' not always) be *attracted towards it*; plainly removing it ſelf out of its Place, to come nearer thereto.

If any Body be *interpoſed between the Glaſs and the ſaid directed Thread*; then the Thread, depriv'd of the directing Force of the *Effluvia*, immediately returns to its *firſt* and *natural Poſition*, *viz.* ſuch as its own Gravity gives it. But if that interpos'd Body *be taken away again*, then (provided the Thread has not remov'd it ſelf too far out of the reach of the *Effluvia*) it will *return again to its central Tendency*, and remain in it, till its Gravity becomes too great for the decreaſing Force of the *Effluvia* to ſupport it any longer in ſuch a Direction.

N.B. I have ſince tried the ſame thing with a *Glaſs Globe*, and found that when the Attrition was made, *it would attract the ſurrounding Threads in all manner of Poſitions, and direct 'em to its Center*. What we may obſerve from hence therefore, is,

See Fig. 7. Plate VII.

Firſt, The *vigorous* and *laſting Action of the Effluvia*, excited by this Attrition; ſeeing the *Tenſion* of the Threads was ſtill continued, tho' the *Friction and Motion* of the Glaſs were both ceaſed.

Secondly, A plain Inſtance of a *Repulſive* and *Attractive* Force. For the Threads *avoided* the Finger (as if acted by ſome *centrifugal* Force) when it approach'd very near their Extremities : And at another diſtance, ſomething like a *centripetal* Force, would cauſe them to incline and *move themſelves towards it.* So that in theſe ſmaller Orbs of Matter, we have ſome little Reſemblances of the Grand Phænomena of the Univerſe.

Thirdly, The *certain Dependence* of this Phænomenon (*viz. the Extenſion of the Threads*) upon the *Action of ſome Matter*, whoſe *Direction is in ſtrait Lines towards the Glaſs.* For upon the Interpoſition of any Body between the Threads and the Glaſs, they loſe their regular Extenſion, and hang as their own Weight cauſes them. And what can ſuch Interpoſition of a Body poſſibly do in this caſe, but only interrupt the Courſe of the Matter which is the Cauſe of the Extenſion, and conſequently make way for the Gravity (which was overcome by a ſuperior Force before) to exert its ſelf again ?

Experiment II.

I Took an *Hemiſpherical Glaſs* (ſuch as is repreſented at *Fig.* 5.) of about ſix Inches Diameter: Into this I convey- *Plate* VII. ed a *Stick*, in manner of an *Axis*, which had the Woollen Threads (formerly made uſe of) tied about it. The Glaſs was ſcrew'd by the *Neck* to one End of a *Spindle*; and being fixed on the *Machine*, the Great *Wheel* *Plate* VII. was turn'd, and the Friction made on the outer Surface of the Glaſs, as uſual. And now the Threads preſented a Phænomenon, not a little pleaſant and ſurprizing to behold; but yet ſuch as I expected and hoped for, in the Contrivance of this particular *Apparatus*. For here was juſt the *Reverſe* of what happen'd when the Semicircular Wire was placed on the Outſide: That is, the Threads *here* iſſu'd like Rays *from* a Center *outwards*; as *there* they *converged* to a Center *within*. The nearer they were to the *Concave* Surface of the Glaſs, the farther their Extremities diverged from one another, in *this* caſe; as in *that* caſe, the nearer they were to the *Convex Surface*, the nearer were their Extremities to one another; in *either* caſe the *Divergency* or *Convergency* was *from* or *to* ſomeCenter in the Axis; and all the difference was, that in the one caſe the Tendency was *from without*, *inwards*; and in the other, 'twas *from within*, *outwards*. The Appearance of the Threads in this laſt Experiment was as is repreſented at *Fig.* 6. *Plate* VII.

And what was very conſiderable farther, was, that while the Threads *lay in this poſition*, like ſo many Rays of a Circle extended; if the Finger were held near the *Outſide* of the Glaſs, a Motion would be communicated to the point of that *Thread which was neareſt within:* So that *by the Motion of the Finger*, the *Thread would be driven any way* before it. And it would ſeem to fly and avoid the Finger, held on any ſide; tho' the convex Surface of the Glaſs were not touch'd by it, by ½ Inch and more (as I have ſometimes ſeen.) Likewiſe, if the Threads were remov'd to the *Outſide*, and the Finger mov'd about within, the Threads would play about with the like motion.

And indeed, *generally* ſpeaking, *the Threads ſeem to avoid the approach of the Finger*; tho' I have ſometimes obſerv'd them *to jump ſuddenly towards it*, at more than an Inch diſtance.

From the Experiment now recited, we may obſerve,

The remarkable *Uniformity* and *Agreement conſpicuous in this Phænomenon*, *with that where the Threads were diſpos'd on the Outſide of the Glaſs.* For in both, the Threads were directed, according to the courſe of the matter; which in one caſe acted *one* way, and in the other the *contrary*. And by comparing both, 'tis very plain, that *there is no more than one and the ſame Cauſe of both.* For the ſame Cauſe which in one Circumſtance (*viz.* when the Threads were plac'd on the *Outſide*) would make them *converge* towards the *Convex* Surface of the Glaſs, would alſo in the contrary Circumſtance, (*viz.* when they were placed on the *Inſide*) make them *diverge* towards the *Concave* Surface.

Again, the Agreement anſwers, not only with reſpect to the Direction of the Threads, but alſo to the *Motion* of them. For in the former Experiment, the Threads would ſometimes *avoid* and ſometimes *incline towards* the Finger: And in the Experiment now mention'd, there were the like Indications both of a *Centrifugal* and *Centripetal* Force.

Experiment III.

I Took a *Glaſs Globe* of nine Inches Diameter, which being *exhauſted* of its Air, I fix'd to a Machine to give Motion to it, perpendicular to the Horizon. And to another Engine I fix'd another Glaſs Globe, plac'd at a diſtance ſomething leſs than an Inch from the former Globe; and *having none of its Air drawn out*. The Machines being ſet to work, I apply'd my naked Hand to the Globe; which was *full of Air*; the Effluvia of which (excited by the Attrition) quickly reach'd the *exhauſted* Globe, and *produc'd a Light on* that part of it which was neareſt the other. Now here was not the Friction of any other Body upon this Glaſs, to raiſe the Effluvia, and produce a Light; there was nothing *but the bare Action of the Effluvia from the other Globe*, which Effluvia ſupplied the Place of a more ſolid Body, and made ſuch an Attrition as was neceſſary to the Production of the Phænomenon. The Light was pretty vigorous, and ſpread it ſelf on the Globe as far as the Effluvia was capable of ſtriking on it. Its Colour was not near ſo much inclined to *Purple*, as it was when the Light was produc'd by the Attrition of the Hand: But it would continue upon the Globe for half a minute or more, after the Motion of the rubb'd Glaſs had ceas'd. On the other Hand, if the rubb'd Glaſs were kept in Motion, the other being at reſt; the Light would die preſently; but immediately recover'd again, upon the firſt Motion given to it.

As an Appendix to this Experiment, I ſhall relate another, much to the ſame purpoſe.

I took a *long Glaſs*, whoſe Air was exhauſted, and which had lain by in that State above ſix Months. After I had rubb'd this Glaſs a little with my Hand, to clear it of all Moiſture on the Surface; I held it over the *unexhauſted Globe*, which was then in Motion; and at the ſame time alſo I gave it (*viz.* the unexhauſted Globe) an Attrition with my Hand; upon which there were immediately large and ſurprizing *Flaſhes of Light* produc'd in the long Glaſs, tho' it neither touch'd the moving Globe, nor was provok'd it ſelf by any immediate *ſenſible* Attrition.

The Matters of Fact afforded by this whole Experiment, are reducible to the following Heads.

The Production of a continued Light on one *Glaſs exhauſted of its Air, by* another, *at a diſtance from it, being rubb'd while it was full of Air:* Both being in Motion but one only rubb'd.

The Continuance of the Light on the exhauſted Glaſs, for ſome time after the Motion of the other Glaſs is at an end.

The ſudden Ceaſing of the Light, when the exhauſted Glaſs is at reſt, tho' the full Glaſs (on which alone the Attrition is made) be kept in Motion.

The Production of an interrupted Flaſhing Light upon an exhauſted Quieſcent Glaſs, held over another unexhauſted one in Motion; the unexhauſted one being rubb'd at the ſame time.

And from hence we may obſerve,

Firſt, *The Force and Vigorous Action of the Effluvia, by which they perform the Office of a ſolid Body.* In other Experiments, Lights were produc'd by the Attrition of *one ſolid Body againſt another:* but here a Light is produc'd by *the Friction of a very ſubtile Fluid upon a Solid.* So that this may come in, amongſt many others, as an inſtance of the powerful Effects of *ſmall* Bodies, when put into brisk and vigorous Motions.

Secondly, *The great Intereſt which the Motion of the exhauſted Glaſs has in the Continuance and Preſervation of the Light:* For if that be ſtopp'd, the Light dies away preſently, tho' the unexhauſted Glaſs be in Motion. And indeed, 'tis evident, that the Action of the Effluvia upon the ſaid exhauſted Glaſs is more extenſive, (and ſo capable of producing more conſiderable Effects,) when that Glaſs is in Motion, and the parts of it conſequently ſucceſſively expos'd, by a quick Revolution, to the ſtroaks of thoſe Effluvia; than when it is at reſt, and the Effluvia beat only on one and the ſame particular part of the Surface.

And this is effectually confirm'd by the Obſervation of the *kind* of *Light produced on the exhauſted Quieſcent long Glaſs*: For that was not a *Continued*, but a *Flaſhing* Light; that is, ſuch as diſappear'd as ſoon as it was produc'd on the Surface of that Quieſcent Glaſs.

Thirdly, *The difference in Degree and Intenſity of the Colour, which the different Circumſtances of the Friction are capable of producing.* For the Light produc'd by the *Attrition of the Effluvia* did not come near, as to the degree of *Purple*, to that Light which was produc'd when the *Attrition was made by the Hand.*

An

An Account of an Experiment, confirming the Production of Light, by the Effluvia of one Glaſs falling on another.

HAving obſerv'd, that the Effluvia of Glaſs when they fell on an exhauſted Glaſs in Motion, would exhibit ſuch an Appearance, as if it were rubb'd by a viſible ſolid Body; I thought this further Confirmation of it would not be unacceptable.

I took a large Receiver, of ſuch a form as is expreſs'd by a a a a; (Fig. 1.) within the Body of which I fix'd another, of ſuch a Form, and in ſuch Poſition, as is repreſented by b b b b. Their Axes were parallel to the Horizon, and fix'd one within another at c c. The outward Surface of the inward Glaſs was at leaſt an Inch diſtant from the inward Surface of the outward one: And they were both turned by two large Wheels d d d d, whoſe *Bands* related to the ſmall Wheels e e e e, fix'd on the Axes of the Glaſſes. Before the Glaſſes were thus adapted to each other, the innermoſt was exhauſted of its Air; and then being ſet as the Figure deſcribes, I order'd that Wheel only to be mov'd which gave Motion to the great Glaſs. The Thought which guided this Pro-

Plate III.

ceſs

ceſs, was this ; that when the Effluvia of the great Glaſs (by the Application of my Hand upon it) ſhould reach the other ; this other, notwithſtanding it was at reſt, would nevertheleſs be influenc'd by the Effluvia, and give a Light.

The Effect anſwer'd my Expectation ; for the Light appear'd, and ſpread it ſelf in numerous branches all over.

This done, I caus'd the other Wheel (*viz.* that which gave motion to the included Glaſs,) to be turn'd ; and then the Light became much more conſiderable, and, I think, the greateſt that has yet been produc'd in any Experiment made on this Subject.

And I doubt not, but 'twould have been much more conſiderable, had the inward Glaſs fitted ſo, as nearly to touch the inward Surface of the outer Glaſs ; the *Effluvia* of which (as it appears to me) would then have been capable of acting with more vigour on the inclos'd exhauſted moving Receiver.

Having in the next Place caus'd both the great Wheels to turn the Glaſſes one and the ſame way, with as equal a degree of Velocity as they could ; I did not find but the Light was then as ſtrong, as when their motions were juſt the reverſe.

So that, (as far as I can perceive) neither the contrariety nor agreement of the Motions does contribute any thing to this Phænomenon ; but Motion it ſelf (without any particular

Rules or Limitations) is abſolutely neceſſary: As this, and the whole Courſe of Experiments on this Head, abundantly ſhews.

I obſerved farther, that though the Effluvia ſeem'd to be equally diſtributed on the outward Surface of the inward moving Glaſs; yet the Light appear'd moſt vigorouſly on that ſide of it next the Attrition. And when either of the Glaſſes was at reſt, the other continuing in motion, (I ſay, *either*; for upon Trial I found very little difference either way;) the Appearance of the Light would remain a conſiderable time within the exhauſted Glaſs, till the Effluvia of the other were no longer capable of acting with a Force upon it, requiſite to produce the Effect.

'Twas not a little ſurprizing alſo to obſerve, that after both Glaſſes had been in motion for ſome time, and the Hand apply'd during that time to the Surface of the outer one, that then, the Motions both ceaſing, and no Light appearing at all, if I did but approach my Hand again near the Surface of the outer Glaſs, there would be Flaſhes of Light (like Lightning) produc'd in the inward Glaſs; juſt as if the Effluvia from the outer Glaſs had been puſh'd with more Force upon it by means of the approaching Hand.

The Matters of Fact afforded by this Experiment, may be compriz'd under the following Heads.

The Production of a Light by the Effluvia of the outer circulating Glaſs falling on the inward quieſcent one.

The Extraordinary Augmentation of the Light, upon the circular Motion of the inner Glaſs, together with the outer one.

The Conſtant and Unvaried State of the Effect, as to the Vigour and Strength of the Light, whether the two Glaſſes were moved the ſame or the contrary Way.

The moſt vigorous Appearance of the Light upon the Side next the Attrition, when both the Glaſſes were moved; notwithſtanding the Effluvia ſeemed equally diſtributed on the Surface of the inward Glaſs.

The Continuance of the Light in the exhauſted inner Glaſs, till the Force of the Effluvia was ſpent; when either Glaſs was in motion, and the other at reſt.

The Flaſhes of Light produced upon the inner Glaſs, by the Approach of the Hand to the outer one, no Light at all appearing before: Both Glaſſes being then quieſcent, tho' both had been in motion, and the outward one rubb'd all the time of the Motion.

Query 1. Why is a Medium ſo very much rarefied, as that in the exhauſted Receiver, requiſite to produce a Light?

Query 2. Does not this Light, produc'd by the Attrition of the Effluvia, ſhew, that Minute and Fluid Bodies, when put in violent motion, are capable of performing the ſame Effects as others of a more groſs and ſolid nature?

SECT. III.

An Experiment ſhewing the Difficulty of ſeparating Two Hemiſpheres, *upon the injecting an* Atmoſphere *of Air on their outward Surfaces, without exhauſting the included Air.*

THE beſt Proof that can be given of the Truth of any Hypotheſis, is, that the Experiments made for that end, do all of 'em and every way agree: That trying Nature on one ſide, and on the other, yet every way ſhe ſtill confeſſes the ſame thing. Thus with reſpect to the nature of *Sounds*; 'tis demonſtrable, that the Air is the proper Vehicle or Medium for the Propagation of them; becauſe Sounds do not only *leſſen and grow weaker*, according to the Degrees of the Air's Rarefaction; but alſo become more *intenſe and ſtrong*, according to the Degrees of its *Condenſation*. And I offer the following Experiment, to ſhew, that we have the very ſame Degree of Certainty of the Preſſure and Gravitation of the Air; in that the very ſame Effect is produc'd, when we make uſe of a *condens'd* Atmoſphere to work againſt *common*

Air, as when we make uſe of *common* Air to work againſt a very much rarefied Medium or *Vacuity*. This Experiment, I hope, will be no leſs than deciſive of the Point ſo long in agitation, and ſet the Truth free even from any poſſibility of being attack'd by the Objections of the Favourers of *Suction*, and the *Funicular* Hypotheſis. 'Tis true, the Doctrine of the Air's preſſure has been fairly and clearly demonſtrated by a great number of Experiments, already made for that purpoſe. But ſtill theſe People have found ſome Shifts and Subterfuges, by which they have made a ſhew of evading the Concluſive Force of the Experiments. They have ſtill had room left to ſay ſomething or other, which how little ſoever it has really been to the purpoſe, has yet ſerv'd to keep the Controverſie alive, and make the Unskilful or Unthinking believe they had ſome Probability on their ſide. Now, to ſhew how unreaſonable thoſe Evaſions hitherto made uſe of, have been; as alſo at once to ſet the Matter in a ſatisfactory Light; I proceeded in the manner following.

I took a ſtrong Glaſs *Receiver*, open, and arm'd with *Braſs Hoops* at top and bottom; in which I plac'd two Braſs *Hemiſpheres*, joyn'd together on a wet Leather at b b b b (the Diameter being $3\frac{1}{2}$ Inches) as alſo a Mercurial *Gage* repreſented by c c c c. To the Braſs *Hoops* were applied two Braſs Plates d d d d, with wet Leathers between them.

See Plate IV

them. To the upper Hemiſphere was ſcrew'd a large *Braſs Wire* e e, that paſs'd through a *Box* of Leathers f f, which was ſcrew'd on the upper Plate; and this Wire could eaſily be mov'd up and down without ſuffering any Air to paſs in along with it. This moveable Wire had a *Cock* g g g g ſcrew'd at the upper part of it, thro' which the Air was to be injected. In this manner were the upper and lower Plates firmly ſcrew'd to the Receiver by the Frame and Pillars h h h h h.

Theſe things thus provided; an Atmoſphere of Air was thrown into the Receiver; the Quantity of which Injection was eaſily diſcover'd by the forementiond Gage c c c c; the Air therein poſſeſſing but half the ſpace it did before. When this was done, the *Syringe* i i was taken off, and an *Iron* with an *Eye*, repreſented by k k, was ſcrew'd on in its place; by which means the whole Apparatus was ſuſpended on a *Triangle* l l l l l. (Note, that the moveable Wire and upper Hemiſphere related to this *Iron*; all the reſt being part of the weight made uſe of to ſeparate them.) After this, the Scale which hung at the Bottom had ſo much weight put into it, that all together made full 140 Pound; and nothing leſs than this weight of 140 Pound, would part the Hemiſpheres; ſo powerful was the Force and Preſſure of the Atmoſphere injected on their outward Surfaces, to hinder that Separation and keep them together. Now how thoſe that eſpouſe the *Funicular* Hypotheſis, or that of *Suction*, will ſolve this from their Principles, I can't imagine! For how is it poſſible that any thing of that kind ſhould take place in the matter before us? How and which way does any *Suction* drive theſe two Hemiſpheres together with ſuch a Force? Or where's any room for a *Funiculus*, that may be imagin'd to be the Cauſe of their Union and Compreſſion? I can't ſee but the Matter of Fact is plainly beyond all Exceptions; and that What I have propos'd, is no leſs than an *Experimentum crucis*. Therefore,

Corollary. From hence *the Doctrine of the Preſſure of the Air is certain.*

For there can't poſſibly be any thing aſſign'd as the Cauſe of this Compreſſion of the Hemiſpheres, but the Preſſures of the external condens'd Air on their Surfaces. For, whatever other Cauſe any one ſhall think fit to aſſign; it may eaſily be ſhewn to be impoſſible, from the Circumſtances of this Experiment. From whence the certainty of the Preſſure of the Air being eſtabliſh'd, I may fairly conclude, that the ſame Principal was the Cauſe of the Compreſſion of the Hemiſpheres in the common *Madgeburg* Experiment alſo. For Nature would not do it by *Preſſure* here, and by *Suction* there: This would be ſuch an unequal acting, that the Wiſdom and Simplicity of Nature is by no means to be blemiſh'd with ſuch an Imputation.

And therefore, I think, I may venture to affirm, That all the Objections that have been made againſt this Doctrine, have (at beſt) been the Reſult of nothing elſe, but fallacious and miſtaken Reaſonings.

However, to prevent all Scruples in Them that may be apt to retain any Doubt of the Air's Preſſure, I ſhall add one or two convincing Circumſtances more.

Firſt, Having caus'd the ſame two Hemiſpheres to be exhauſted of their Air, and none but the common open Air being about their outward Surfaces; I found that the ſame weight was requir'd to ſeparate them then, as was requir'd for their ſeparation when they were full of common Air and had an Atmoſphere of Air condens'd on their outward Superficies.

Secondly, Having exhauſted the included Air, I injected an Atmoſphere (the ſame quantity as in the former Experiment) on the external Surfaces of the Hemiſpheres; and then I found that 280 Pound (which was double the weight before requir'd) was not ſufficient to ſeparate them: I was unwilling to add more weight (tho' I knew a ſmall addition muſt have done it) for fear of breaking ſome of the weaker parts of the Machine, which might have been in danger by the fall of ſuch a weight; the Experiment being full and concluſive without it.

Now, what can be a plainer Demonſtration, even to Senſe, of the Preſſure of the Air, than this? Here was no greater quantity of Air injected on the Outſide of the Hemiſpheres, than when the Common Air was left within them; and yet they were preſs'd together by a Force above twice as great as that in the former Caſe: For the Force that compreſſes, is always proportionable to the Weight requiſite to make the Separation. Therefore that ſame *Air*, contiguous to their outward Surfaces, *preſs'd* againſt thoſe outward Surfaces; and that with a Force above twice as great as it did in the former Caſe.

This Property of the Air thereſore is certain beyond all diſpute.

SECT. IV.

An Experiment concerning the Proportion of the Weight of Air, to the Weight of an equal Bulk of Water, without knowing the abſolute Quantity of either.

I Took a Bottle which held more than three Gallons; but how much more, we have no occaſion at preſent to take notice of; and of a Form ſomething Oval: Which Figure I made choice of, for the advantage of its more eaſie Libration in Water. Into this Bottle I put as much Lead as would ſerve to ſink it below the Surface of the Water. And the reaſon why I choſe rather to have the Weight of Lead inclos'd within the Bottle, than fix'd any where on the Outſide, was, to prevent the Inconveniencies which in the latter caſe muſt needs have aroſe from *Bubbles of Air:* For theſe *Bubbles* would have inevitably adher'd to, and lurk'd in great plenty about the Body of the Weight, had it been plac'd on the Outſide: Which muſt have caus'd ſome Errors in the Computations of an *Experiment* that requir'd ſo much exactneſs and nicety.

Theſe things thus provided, the Bottle, containing Common Air ſo clos'd up, was by a Wire ſuſpended in the Water, at one end of a very good *Balance*; and *was counterpoiz'd in the Water by a Weight of* 358 *Grains and an half* in the oppoſite Scale.

Then being taken out of the Water, and ſcrew'd to the *Pump*, in five Minutes time it was pretty well exhauſted; the Mercury in the *Gage* ſtanding at near 29 Inches and an half: After which, having turn'd a Cock that ſcrew'd both to the Bottle and the Pump, and ſo prevented the Air's return into it again, it was taken off from the Pump, and ſuſpended as before, at one end of the Balance in the Water.

And now the Weight of it was but 175 Grains and an half: Which therefore ſubſtracted from 358 Grains and an half (*the Weight of the Bottle with the inclos'd Air, before it had been apply'd to the Air-Pump*) gave for the difference 183 Grains: Which Difference muſt conſequently be the Weight of the Quantity of Air drawn from the Bottle by the Pump.

Having thus determin'd the Weight of the exhauſted Air, a Cock was open'd under Water; upon which the Water was at firſt impell'd with a conſiderable Violence into the Bottle (though this Force abated gradually afterwards;) and continued to ruſh in, till ſuch a Quantity was enter'd, as was equal to the Bulk of the Air withdrawn. And then the Bottle being examin'd by the Balance again, was found to weigh 162132 Grains: From which ſubſtracting 175 Grains and an half (*the Weight of the Bottle, with the ſmall Remainder of included Air, after it was taken from the Air-Pump*) there remains 161956 Grains and an half, for *the Weight of a Maſſe of Water equal in Bulk to the Quantity of Air exhauſted. So that the Proportion of the Weights of two equal Bulks of Air and Water, is as* 183 to 161956 an an half; which is as 1 to 885 $\frac{1}{132}$; or, in round Numbers, as 1 to 885.

And there are two things particularly obſervable in this Experiment.

Firſt. That in making it after this manner, *one need not be very ſollicitous about a nice and accurate Exhauſtion of the Receiver.* The Succeſs of this Experiment does not all depend upon it: For to what degree ſoever the Exhauſtion be made, it muſt ſtill anſwer in proportion to the Quantity taken out. Neither can any more Water poſſibly enter into the Receiver, than what will juſt ſupply the place, and fill up the room deſerted by the exhauſted Air.

But, *Secondly*, *The Seaſon of the Year is to be conſider'd in making of this Experiment.* I made it in the warm Month of *May*; the Mercury in the Barometer ſtanding at the ſame time at 29 $\frac{7}{10}$ Inches.

From whence 'tis reaſonable to conclude, that a ſenſible Difference would ariſe, were it to be tried in the Months of *December* or *January*, when the State and Conſtitution of the Air, is uſually different from what 'tis in the forementioned Month.

SECT. V.

An Experiment, ſhewing, that the Aſcent of Liquids in ſmall Tubes open at both Ends, is the ſame in Vacuo *as in the open Air.*

I Took three *ſmall Tubes* of different Diameters, and fix'd them in a Piece of Cork, in an exact *perpendicular* Poſition: Alſo their lower Orifices were ſet as nicely in one and the ſame *Horizontal Plane* as I could.

This Cork I faſten'd to a *Wire*, which paſſed thro' ſome *Collars* of *Leather*, included in a *Box* on the upper Plate of the Receiver; by which means I could at pleaſure elevate or depreſs the ſmall Tubes, without any danger of the Air's getting in.

See Fig. 3. *Plate* II.

Then ſome Water, which was tinged with a deep Colour, being ſet on the lower Plate, the ſmall Tubes, which had never been wetted, were drawn to the upper Part of the Receiver by the help of the forementioned Wire. And the Air being exhauſted, the ſaid Tubes were made to deſcend (by the ſame Wire which drew them up) till their lower Orifices were *immers'd juſt below the Surface* of the tinged Liquid. This was no ſooner done, but the Liquor *mounted up in each of them to a conſiderable Height above its Surface in the Baſon*; but *higher in the ſmaller Tubes than in the larger ones.*

And, what was farther worth notice, the Liquid ſo elevated, was alſo retain'd in theſe ſmall Tubes, tho' their lower Orifices were lifted out of the Water.

Upon the Re-admiſſion of the Air, the Fluid ſtood at the very ſame elevation, in each of the Tubes, as it did before. What height ſoever it mounted to in *Vacuo*; it preſerv'd the ſame without the leaſt ſenſible Alteration, when the Air was permitted to have free acceſs to it again.

So that the matter of Fact obſervable in this Experiment, is contained under theſe two Heads:

Firſt, That *the Fluid roſe in the ſmall Tubes in the exhauſted Receiver.*

Secondly, That *the Admiſſion of the Air made no change in the Height.*

From both which put together, it follows directly (as I take it) that *the Air is not the Cauſe of the Riſe of Liquids in ſmall*

Tubes. For if it be, how then does the Liquid come to riſe in the exhauſted Receiver?

If it be ſaid, that the *Vacuum* is not a perfect one, and there is ſome portion of Air left in the Receiver; I enquire then, if that ſmall portion of very much weaken'd Air left in the Receiver was ſufficient to raiſe the Fluid to ſuch a height, would not a new force of Air let in, have made an Alteration, and carried it to a greater height? If the Liquid riſes by means of that Air left in the Receiver, 'tis certainly by virtue of its preſſure on the Surface of the ſtagnant Fluid, into which the Orifice of the ſmall Tube is put: And therefore when that Preſſure is ſtrengthen'd by the force of a new Quantity of Air admitted in, this more powerful Cauſe ſhould produce a greater Effect; and the Fluid ſhould riſe higher: Which yet it does not, but keeps at the ſame unvaried Height.

From whence, I think, I may without ſcruple, conclude, that the Air has nothing at all to do in this matter: For 'tis plain Fact, that the Abſence of it does not hinder, nor its Preſence help the Effect: And what neither helps nor hinders, no Philoſophy in the World will allow to be a Cauſe.

Beſides, if to the Matter of fact afforded by this Experiment, we add a Conſideration or two more, it will render the Argument yet more ſenſibly convincing. For Liquids will riſe in ſmall Tubes, *in the open Air* (as we ſee every day :) Again, they will keep the ſame Height they have riſen to in *Pleno*, after the Air be drawn out, and they be left in *Vacuo*.

Now, join theſe two Conſiderations with the former, and I think it renders the Evidence as compleat as can be deſir'd : For if Fluids will riſe (in ſmall Tubes) *in the open Air*, and alſo in the empty Receiver ; and if they will keep their Height they roſe to in *Pleno*, tho' you make a *Vacuum* ; and keep their Height they roſe to in *Vacuo*, tho' you make a *Plenum* ; then 'tis manifeſt, that this Phænomenon is abſolutely indifferent, with reſpect either to the *Preſence* and *Action*, or to the *Abſence* and *Non-Action* of the Air ; and therefore that the Air it ſelf cannot be the Cauſe of it.

I would farther add here an Obſervation or two that I have made, *concerning the Properties of theſe ſmall Tubes.*

Firſt, If a ſmall Tube be bent into the Form of a Syphon, then *obſerve how high the Liquid would of it ſelf riſe in the ſhorter Leg of ſuch a Syphon, if it were immers'd in Water :* For *the Orifice of that ſhorter Leg of ſuch a Tube, muſt always be at leaſt as far below the Surface of the ſtagnant Fluid, as that Height amounts to, before it will run out at the longer Leg.*

Which is a pretty remarkable difference between theſe ſmall and the vulgar larger Syphons. For in Thoſe we are not limited to any certain and particular Depth,at which the Orifice of the ſhorter Leg muſt be plac'd, before the Water will run out at the longer: But in Syphons made of very minute Tubes, ſuch as Liquids will ſpontaneouſly elevate themſelves in, there is required a certain Depth at leaſt, for the Immerſion of the Orifice of the ſhorter Leg; ſince all Depths leſs than the Height of the ſpontaneous Aſcent, will cauſe no Effuſion of the Liquid out of the Orifice of the longer Leg.

And from hence 'tis an obvious *Corollary, That in ſmall Syphons, whoſe Orifices are of different Diameters, thoſe need to be plunged to the leaſt Depth* (for cauſing the Water to run out at the other Leg) *whoſe Orifices are the largeſt.* For in Tubes of the largeſt Orifices, the Fluid aſcends of it ſelf to the leaſt Height.

Wherefore ſince in order to the running of the Liquor, the Depth of the Immerſion muſt be (at leaſt) equal to the Height of the ſpontaneous Aſcent; it apparently follows, that Syphons of a larger Orifice will run at a leſs Depth of the ſhorter Leg's Immerſion below the Surface of the ſtagnant Liquid, than thoſe of a narrower Orifice will do.

Secondly, By Trials made with Tubes of various Sizes and Proportions, I ſound this to be a conſtant and perpetual Rule, *viz. That ſo much of the Liquor would always remain ſuſpended in them, when lifted up out of the ſtagnant Fluid, as would be elevated above the Surface of it, while they were immers'd in it.*

From whence it follows, that ſome Cauſe (whatſoever it be) which concurrs to the elevating of the Fluid into the Tube, while it is immers'd, does contribute as powerfully to keep it at the ſame Height, after the Tube is taken out of the ſtagnant Liquid.

An Acconnt of an Experiment, concerning the quantity of Air produced from a certain quantity of Gun-powder fired in Common Air.

I Took a fine Glaſs Tube, about 36 Inches long, the Diameter of whoſe *Bore* was near ¾ of an Inch. The upper Orifice had a *Ferrel*, ſoder'd to a *Screw* cemented on it, to which was ſcrew'd a *Cock:* The lower Orifice was quite naked and open, it being no way needful to have any guard ſet on that part. Near the upper part of this Tube, in the Inſide, was fix'd a piece of *Cork*, notch'd on its edges, to give the greater ſcope and liberty to the Exploſion. The Cork had a ſmall *Cavity* in the middle of it, the better to hold Gun-powder, which was let down upon it thro' a ſmall Glaſs *Funnel*, before the Cock was ſcrew'd on. And in this manner, was the lower Orifice of the Tube plunged under the Surface of the Water contain'd in a Veſſel. The Cock being then ſcrew'd on, and open; 'twas an eaſie matter, by ſucking at it with ones Mouth to remove the Preſſure of the *inward* Air: By which means the Preſſure of the *outward* Air would raiſe the Water in it to any determinate height. And the

Tube being accurately graduated by a *Flie* on its Outſide, one might meaſure the Quantity of the Aſcent with all the eaſe and exactneſs imaginable.

When the Water had got up to the intended mark, the Cock was turn'd, which kept it ſuſpended there. And a *Burning Glaſs* being applied, the Rays, were drawn to a *Focus* upon the Gun-powder; which fir'd it very quickly, and forc'd the Water down with a great violence; but it roſe again ſuddenly afterwards; however, it reſted ſo far below the mark it ſtood at before the Exploſion, as was equal to the Quantity of *ſeeming Air* produc'd thereby. The Quantity of Gun-powder uſed in this Experiment, was exactly one Grain. And I found that the Quantity of ſpace the Water had deſerted juſt after the Exploſion, was ſuch as would contain nearly a cubical Inch of Gun-powder, the weight of which was equal to 222 Grains. So that 222 Grains of the ſame Powder, ſeem (as ſoon as fired) to produce ſomething which poſſeſſes the ſpace of ſo many cubical Inches of Air. *Now whether the ſpace deſerted by the Water is poſſeſs'd by a Body of the ſame weight and denſity, or which has the ſame qualities with Common Air*, I dare not determine; ſince an Experiment I lately made *(to try how much the Heat produc'd by the Exploſion of the Gun-powder, might contribute to the largeneſs of the ſpace deſerted by the Water)* ſeems to conclude otherwiſe. That matter was thus:

The whole ſpace deſerted by the Water, was divided (length ways) into 20 equal parts. Now an Hour after the firing of the Gunpowder, the Water had aſcended about $\frac{2}{20}$ of the whole ſpace, which was 2 Inches, accurately ſpeaking, or ſuppoſe it to be 2½ Inches. At the diſtance of two Hours after the firing, it had got up to near $\frac{4}{20}$ of the ſame. And then I judg'd it might have been of an equal Temper with the external Air, (and conſequently not have given way to the Liquid to have riſen any higher.) But continuing the Experiment ſtill farther, I found (to my great ſurprize) that two Hours after the laſt Obſervation, the Water had mounted to about $\frac{5}{20}$ of the ſpace. Next Morning (which was about 18 Hours diſtance) it had reach'd near $\frac{10}{20}$, or $\frac{1}{2}$ the firſt deſerted ſpace. And continuing thus to riſe, I found that at the end of 12 days, the Water had aſcended to ſomething more than $\frac{17}{20}$. And at 18 Days it had reach'd 19 of the 20 parts at firſt deſerted by it. And *at this ſtation it reſted, continuing there for* 8 *days, without alteration.*

I would *obſerve* one or two things here, before I make any *Deductions* from this Experiment.

Firſt, That *I all-along conſider'd the Temperature of the Air, and found that it contributed nothing at all to this odd Phænomenon.*

Secondly, That tho' the Account here given *may ſeem to thwart ſome Accounts formerly given about the firing of Gun-powder in* Vacuo: yet, conſidering the *vaſt difference of the Mediums* in which the Experiments were made, they may be reconcil'd to one another. For when the Gun-powder was fired in ſo thin a Medium as *a near Approach to a Vacuum*, 'tis plain that the Air remaining in the *Receiver* could ſuffer no more by the Exploſion, than in proportion to its Quantity: Which Quantity being ſo very inconſiderable, the Effects could but anſwer accordingly. Beſides, were thoſe Experiments to be repeated again, ſome Occurrences, which at the firſt Trials might paſs unheeded, would perhaps be taken notice of, which might render all more eaſie and agreeable than now it ſeems to be.

Corol. 1. *'Tis plain, that the matter produc'd by the Exploſion, (whatever it were,) was of a ſpringy contractile Nature, and but very little in quantity, in proportion to the ſpace which it at firſt forc'd the Water out of.* For it reduced it ſelf at laſt into the 20th part of the Space deſerted by the Water; that is, into the 20th part of a Space equal in content to a cubical Inch, or 222 Grains of Gun-powder. So that it was in bulk equal to no more than about 11 Grains, which is nearly the 23d part of the aforeſaid number.

Corol. 2. *The Contraction or Reſtitution of this ſpringy matter was not Equable an Uniform, nor indeed, (as far as I could find,) according to any regular Law; but very diſproportional with reſpect to the Times.* For the degrees of the Contraction would be as *the Spaces (reciprocally)* into which the Matter was reduc'd, by that Contraction; and the Spaces into which the Matter was reduc'd, were exactly diſcover'd by the Aſcent of the Water. Now, at one Hour (after the firing of the Powder) the Water had aſcended 2 of the Diviſions; at 2 Hours, 4; at 4 Hours, 5; at 18 Hours, 10; at 270 Hours, 17; at 432 Hours, 19; where it ſtood without alteration for the Space of 8 Days: So that the encreaſe of the Water's Aſcent, and conſequently the Reſtitution of this contractile Matter, was very far from being equable and regular. At firſt it anſwer'd in proportion to the Times; but afterwards varied enormouſly from that Law; as is apparent. For in the firſt 4 Hours, it roſe 5 Diviſions; and at the End of 18 Hours, it had riſen but 10: So that in the laſt 14 Hours of the 18, it had gain'd no more ſpace than it had the firſt 4 Hours; which was 5 Diviſions. And ſo in the reſt that follow, the difference was ſtill greater.

N. B. Whether the Matter which was the Cauſe of this Phænomenon, were real Common Air or no, is what I will not venture to determine : 'Tis ſufficient for my purpoſe to have propos'd the Matter of Fact, and to have prov'd that this Matter had ſuch and ſuch Properties. To me it ſeems highly probable that it ſhould be an *Heterogeneous Compound of Common Air, of ſome Aerial parts reſiding in the Gun-powder, and of the Nitrous and Sulphureous Matter* which are Ingredients in the ſame. For all theſe muſt needs (I ſhould think) be violently hurried and mix'd together, upon the Exploſion ; and conſequently, muſt all joyntly compoſe one Heterogeneous Medium, which afterwards diſplay'd it ſelf by ſuch Effects as I have now mentioned.

The *Elaſtick* or *Self-reſtoring Property* of this Matter, ſeems chiefly to be owing to the *Air* contain'd therein. And that the *Springs of Air may be ſo diſordered by a violent impulſe, as to require Time to recover their natural State again*, will be very clearly made out by the following Experiment.

An Experiment about diſturbing the Spring of the Air.

I Took my *Condenſing Engine*, and put about half a Pint of Water into the bottom part of its *Braſs Receiver*. Then the upper part being ſtrongly ſcrew'd on, I threw into it with the *Syringe* about 3 or 4 Atmoſpheres of Air, as near as I could gueſs: And in this ſtate I ſuffer'd it to remain ſomething more than an Hour. Then letting out as much of the Air (by taking off the *Syringe*) as would readily go away, I preſently ſcrew'd on in its room a *Box of Leather Collars*, thro' which there paſs'd a ſmall *Glaſs Tube*, open at both ends, whoſe lower Orifice was plunged under the Surface of the included Water. After this, in a very little time, I found the Water had aſcended near a Foot in the Tube; and it continued riſing, till it had reach'd near 16 Inches.

Upon a *Repetition of this Experiment*, I let the Air remain in that ſtate of Compreſſion for about 18 Hours. And then (proceeding in all reſpects as before) I found the included Water aſcend gradually in the Tube; and obſerv'd That motion for the ſpace of 6 Hours:

At which time the little Tube was accidentally broken, and ſo farther Obſervations for that time prevented. But however from hence we may infer,

Corol. 1. That *the Springs of Air may be ſo diſturb'd by violent Impulſes, or ſtrong Compreſſions, as to require conſiderable time to recover their Natural Tone and Temper again.*

Corol. 2. *And the Times which the Springs of Air will require for their compleat reſtitution, will be greater or leſs, according as the Forces by which they are thus impell'd or compreſs'd, are greater or leſs; or according as the Times during which they continue in that violent ſtate, are greater or leſs.* That it ſhould be ſo, is perfectly conſonant to the Nature and Properties of the Air; and that it is ſo, the Circumſtances of the Experiment evince. For when the Air had lain compreſs'd for about 18 Hours, the Aſcent of the Water was more ſlow and deliberate; it creeping up gradually for the Space of 6 Hours together. But when it had ſuffer'd the Compreſſion but for the Space of an Hour; the Water advanc'd upwards in the Tube ſo faſt, that in a very ſmall time it had mounted a whole Foot. And there's no reaſon at all to doubt, but that longer Time, and more valid Compreſſions, would produce ſtill greater and more conſiderable Effects, as to the times of the Springs recovering themſelves.

And hence therefore, *Were this Proportion once ſettled and eſtabliſh'd by a ſufficient Number of Experiments; from the Air's foregoing Compreſſion, one might limit and foretell the Motions of the included Liquid; and,* vice verſâ, *from the Motion of the Liquid, one might infer the Air's foregoing Compreſſion.*

Corol. 3. *Wherever therefore (in any Bodies whatſoever) the included Springs of Air ſuffer any ſuch Compreſſions as theſe are, or any anſwerable to theſe; and there be any contiguous Fluid Matter for theſe Springs, as they reſtore themſelves to preſs upon; theſe ſame Effects muſt follow: That is, the Fluids, muſt be put in motion, and advance according as the others preſs them; and if they were in any ſort of motion before, that Motion muſt be alter'd, and either accelerated or retarded, according as the Courſe and Direction of the Fluid be either with or againſt that of the Aerial Springs, while they are in this Action of Self-reſtitution. And perhaps ſeveral Phænomena, as well in Plants and Animal Bodies, as in other Syſtems of Matter in our Globe,* may owe their true Riſe to ſome ſuch Cauſe as this. 'Tis certainly a *poſſible* Cauſe, from the Laws of *Staticks*; and perhaps, inquiſitive Perſons may find out ſome Effects of it.

An Account of an Experiment, ſhewing the Cauſe of the Deſcent *of the* Mercury *in the* Barometer, *in a Storm.*

'TWAS obſervable in the late violent Storm of Wind, that the Mercury in the Barometer did not only ſubſide very conſiderably; but alſo that, upon extraordinary Guſts, there were ſenſible and manifeſt Vibrations of it in the Tube.

Now, to account both for the *Depreſſions* and *Vibrations* of the Mercury in theſe and ſuch like Caſes, I contrived the following

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Experiment.

HAving provided a *Receiver* A, which held about 16 Quarts, I compreſs'd in it about three or four times its natural Content of Air, by the help of the *Syringe* B: Which Inſtrument is for that purpoſe ſcrew'd on at c c.

See Fig. 3. Plate V.

This done, and the *Stop-cock* D, fitted to this Receiver, being turn'd; the Syringe was taken off, and a Braſs *Pipe* about half an Inch Diameter, repreſented by E E, ſcrew'd on in its room.

This Pipe is inſerted into a well fitted *Braſs Socket*, which is fixed in a ſquare *Piece of Wood* F F, and that directly againſt a *Tube*, G G, which enters the ſame Piece of Wood, and is placed parallel to the Horizon. Now, out of the ſame Piece of Wood there riſes a naked Barometer, H H, the Ciſtern of which lies open to the Paſſage leading from the foreſaid *Pipe* E, to the *Horizontal Tube* G.

Beſides this, out of that ſame *Piece* F, there proceeds another Horizontal *Pipe* I I, which runs to another ſquare *Piece of Wood* K K, placed at the diſtance of three Foot from the former.

And this ſecond *Piece* K, has likewiſe *a Barometer* ariſing out of it, L L; the *Ciſtern* of which is alſo open to the Horizontal Tube I, and by that means maintains a Communication with the open Ciſtern of the other Barometer.

All the Parts of the Machine being thus diſpos'd, the Stop-cock was turn'd; and the condens'd Air ruſh'd out of the Receiver with a great Force, thro' the Pipe E, which diſcharg'd it into the Horizontal Tube G.

The Reſult of which was, that *this rapid Current of Air ſo leſſen'd the Preſſure of the Atmoſphere upon the ſtagnant Mercury in the Ciſterns of the reſpective Barometers, that the Mercury was made thereby to deſcend at leaſt two Inches.* Nay, that Barometer L L, which was three Foot diſtant from the Aerial Stream, *was equally affected as the nearer one* H H; the Mercury ſubſiding nearly to a juſt Paralleliſm in both.

And 'tis farther obſervable, that *as the Force of the Current of Air diminiſh'd, ſo the Weight of the Atmoſphere recover'd its Strength again, and forc'd the Mercury in the Barometers to a gradual Aſcent.* Hence,

Corol. 1. *We have a clear and natural Account of the Deſcent and Vibrations of the Mercury, in violent Storms and Hurricanes.* For the mighty Force of thoſe Guſts of Wind, will weaken the Preſſure of the Incumbent Atmoſpherick Columns ; from whence a Deſcent of the Mercury muſt neceſſarily follow. And the interrupted uneven Action of thoſe *Blaſts*, or the quick and ſudden Returns of them, are capable of producing and continuing the Vibratory Motions (*i. e.* the quick Aſcents and Deſcents) of the ſame.

Corol. 2: *Not only the different* Forces, *but alſo the different* Directions *of Winds, are capable of producing a Difference in the ſubſiding of the Mercury.*

That Winds of different *Strengths* ſhould produce proportional Effects, in breaking the Preſſure of the Atmoſpherick Columns ; is as reaſonable, as that a greater Power ſhould ſuſtain a *greater* Pondus, or take off more of the Preſſure of the *ſame* Pondus, than a leſs can do. And 'tis no leſs evident, that the different *Directions* of the Aerial Currents, muſt be attended with different Effects too ; thoſe whoſe Courſe is from the *lower*, towards the *higher* Regions of the Atmoſphere, having both *ſhorter* and *finer* Columns of Air to encounter the Force of, than thoſe whoſe Courſe is from the *higher*, to the *lower*, where the Columns have both more *Length* and *Denſity* too.

I ſpeak this with reſpect to the real Difference of the Effect in it ſelf, and not as to the outward Senſibleneſs of it to us ; for Changes may happen, when we can't or don't obſerve them. But all other Circumſtances being alike, this Circumſtance of different *Direction* muſt, I doubt not, produce a real Variety. And if all other Circumſtances are not alike, then the Proportions of the Effects will be had from the Compoſition of the Proportions (either *direct*, *reciprocal*, or *both together*) of the Cauſes which make thoſe different Circumſtances.

Corol. 3. *Strong Winds may affect the Animal Oeconomy, upon this very Account, of their altering the Preſſure of the Atmoſphere.*

Let us imagine a number of Pipes or Canals, of an *elaſtick flexible* nature, replete with ſome Fluid; the Preſſure of the incumbent Atmoſphere, is in this Caſe to be conſider'd as a *Pondus*, acting againſt the Force of theſe Elaſtick Canals, with that of their contain'd Fluid. And according to *Mechanick* Laws, theſe diſtractile Tubes will be ſo far compreſs'd by that incumbent Weight till a juſt Equilibrium is produc'd between the two Antagoniſt Forces; and then they will preſerve themſelves on both ſides in that State, till ſome farther alteration ſhall happen, to leſſen the *Momentum* either of one or the other. If therefore the Preſſure of the incumbent Columns of Air be in any meaſure broken or taken off; the Canals will reſtore themſelves ſo far forth by their Elaſticity, till the *Momentum* of their Retinency becomes equal to that of the diminiſh'd Preſſure. From whence 'tis manifeſt, (ſuppoſing the contain'd Fluid to be in Motion) that the Rate of the Progreſs of that Fluid, muſt needs undergo an alteration, in proportion to that of the Change made in the external Preſſure. For the ſtronger Preſſure will ſtraiten the Canals, and conſequently encreaſe the Velocity of the Fluid: As, on the other Hand, the more feeble Preſſure will give way to the Canals endeavouring to enlarge themſelves; and by that means will contribute to the more ſlow and deliberate Motion of the Fluid. The Application of all which to the Bodies of Animals is very obvious and eaſie: For they are nothing more than ſo many Complications of branching Canals, and tender flexible Membranes, eaſily yielding to an external Preſſure or Pulſion, and capable of reſtoring themſelves by their innate Spring. The great Weight of the Atmoſphere is always preſſing down upon theſe Machines; and 'tis the Spring and Retinency of their parts, which is the counter-balance to it, and preſerves them from receiving injuries by it. The Veſſels conſequently, which ſerve for the Diſtribution of the Animal Fluids, being differently ſtraiten'd and compreſs'd by the various Weights of the incumbent Atmoſphere; the Liquids are affected with new and different Degrees of Velocity. And therefore, when any extraordinary Changes happen in the Weight and Preſſure of the Atmoſphere, there muſt be (*cæteris paribus*) as conſiderable Changes in the Motions of the Fluids. But violent Guſts of Wind, Hurricanes, and the like, will neceſſarily produce very great differences in the Weight of the incumbent Atmoſphere: And therefore, I ſay, very conſiderable Alterations may be made in the Motions of the Liquids in Animal Bodies, by ſuch Cauſes as theſe.

From whence it follows, that whatever Changes are poſſible to be produc'd in Animal Bodies, by the mere Alteration of the Velocity of the Liquids; are, in ſome meaſure at leaſt, producible by very ſtrong and violent Winds: And theſe Changes in the Animal Oeconomy (*viz.* that depend upon the alter'd Velocities of Fluids) are not a few.

Corol. 4. The Weight of the Atmoſphere being diminiſh'd in one place, it is alſo as much diminiſh'd at the ſame time in another Place, which holds a Communication with the former.

This is plainly viſible in the Experiment it ſelf. For the Force of the Air in the Ciſtern in the Incloſure F, being broken by the violent Eruption out of the Pipe E; that alſo of the Ciſtern in the Incloſure K, which communicated with the former, was ſo far diminiſh'd too, that the Mercury (whoſe Height depended upon it) ſubſided in that Barometer juſt (or near) as much as in the other. And the like Effects muſt be produced otherwhere, when the Circumſtances anſwer to theſe here.

An Account of ſome Experiments made on the Phoſphorus *in* Vacuo.

Experiment I.

HAving provided a dark Room, I drew ſome Lines with the *Phoſphorus* on a Piece of *Blue* Paper: This immediately became luminous in the *open* Air, and appeared with a Wave-like Undulating Motion. But being placed in a *Receiver*, after ſome few Exſuctions, the Undulation ceaſed; but the Light ſeem'd conſiderably augmented. The Receiver being farther exhauſted, it grew ſtill brighter, and continued with that Increaſe of Luſtre, till an Admiſſion of Air, which did ſenſibly diminiſh it. This Decay of the Light was alſo gradual, anſwering to the gradual Admiſſion of the Air: Tho' upon the Repetition of the Experiment, ſome Perſons preſent believ'd the Light not altogether ſo brisk and vivid as at firſt.

Experiment II.

I Took two or three ſmall Pieces of *Phoſphorus*; which being put into a Glaſs Diſh, I mix'd with it a ſmall Quantity of *Oil of Vitriol*, *Oil of Tartar per Deliquium*, and *Oil of Cloves*. This Mixture fir'd the *Phoſphorus* in the open Air; but it was extinguiſh'd again by the Addition of a little common Water.

This Preparation being included in a Receiver, very little Light appear'd. But the Air being exhauſted, it became very apparent, with a briſk and vigorous Emiſſion of Steams. The Ingredients of this Compoſition in the Diſh, ſeem'd at the ſame time to reſemble a boiling Flame, and exhibited a copious Light; ſo that ſeveral Objects that were near, became very diſtinguſhaible. And this lucid Appearance continu'd till the Air was admitted: But upon that, all became opaque and dark. Neither would ſhaking the Engine (by which means the Mixture it ſelf underwent an Agitation) produce any ſenſible Recovery of the Light.

Experiment III.

HAving put a ſmall Quantity of the fore-mention'd Compoſition into a Bottle with a narrow Neck, I included it in a Receiver; and it yielded then but very little Light. But upon the Exhauſtion of the Air, it began to be luminous; and the Light improv'd in proportion to the encreaſing Rarefaction of the Air, iſſuing out of the Bottle in a Pyramidical Form.

At laſt (tho' the Receiver was well exhauſted) the Steams then emitted, did fairly aſcend in that very rare and thin Medium, and reach'd the upper Parts of the Receiver (which was not a tall one) but deſcended down again by the Sides of it. Upon the Re-admiſſion of the Air, the Light perfectly vaniſh'd; and it would have been in vain, as I have often try'd, to have expected the Recovery of it in the open Air.

Theſe three Experiments do all exactly agree, in confirming this Concluſion, *viz. That the Phoſphorus-Light is improv'd by the Rarefaction of the Air.*

Common Air is therefore ſome way or other an Impediment to the Action of thoſe Steams on which the *Light-giving* Quality depends. It remains therefore to be enquir'd, By virtue of what Property of the Air it is, that the Action of the Luminous Steams is thus impeded?

And I think it highly reaſonable to conclude, That *the Preſſure of the Atmoſphere is that Impediment upon the Luminous Matter in this caſe.* For the Air, as a *Pondus* incumbent on the Body which contains the Luminous Matter, urging the Surface on all ſides, and rendring it conſequently more cloſe and compact; the lucid Steams, whoſe Efforts and Endeavours to expand themſelves, cannot balance this over-ruling Preſſure, are by that means kept in, and cannot be diſcharg'd.

Yet I do not ſee, that we can argue here from the Denſity and Gravity of the Air, conſider'd as a *Medium.* For if the Luminous Matter were ſpecifically *heavier* than common Air, it would much more be heavier than rarefied Air in an Approach to a *Vacuum*; and conſequently the Steams could not riſe, nor the Light appear, (much leſs improve) in the Exhauſted Receiver, as we ſee it does.

Again: If any thing depended on the Luminous Matter's being ſpecifically *lighter* than Common Air, yet in the ſeveral Degrees of Rarefaction approaching towards a Vacuity, there would be Mediums produc'd, approaching ſtill nearer and nearer to the ſpecifick Gravity of the Luminous Matter. And conſequently, as the Receiver is more and more exhauſted, ſo the Fumes ſhould be diſcharg'd in leſs Plenty, and aſcend with leſs Velocity. Whereas on the contrary, they riſe more copiouſly; and, the Light being more bright and vivid too, 'tis plain that they expand themſelves, not with leſs, but greater Force. And therefore I think the Gravity of the Air, as a *Medium*, has very little, if any thing at all, to do in this Affair. But the Preſſure or Gravity of the Air, as a *Pondus*, I believe, will account for it; and, as far as I can ſee, is the only Property that will do ſo.

I would remark here farther, particularly with regard to the Third Experiment, that the *Phoſphorus* Steams were apparently ſpecifically heavier than the Medium produc'd in the Receiver by the laſt Exſuction. For they did *deſcend* in that Medium.

And from hence I may ſecurely infer, that they did not aſcend in that Medium by *Hydroſtatical Laws*, but by the mere Impetus of their own Vibratory Expanſive Motion, or the Force with which they were emitted from the Body which contain'd them, upon the removal of the Preſſure which was before an Impediment to their Diſcharge. For, that Impetus being ſpent, or overpower'd by their Gravity, they neceſſarily deſcended again by virtue of that *Law*, which obtains in all Portions of Matter of all ſorts whatever.

An Account of ſome Experiments made about the Propagation of Sounds *in* Condens'd *and in* Rarefy'd *Air.*

EXPERIMENT I.

Concerning the Propagation of Sound *in* Condens'd Air.

A Bell being included in a *Braſs Receiver*, was placed at one End of a Room about fifty Yards in Length: At the other End of which ſome Perſons ſtood to obſerve the Sound.

Before any Air at all was injected, the Bell, by ſhaking the Receiver, *might be heard at that Diſtance, though not without diligent attending to it.*

When one Atmoſphere was injected (*if I may take the liberty to uſe that Expreſſion*) the Bell being ſhaken, as before, the Sound was obſerv'd to be very *ſenſibly augmented.*

When two Atmoſpheres were injected, there was manifeſtly *a much more conſiderable Improvement of the Sound.*

But upon the Intruſion of the third, fourth and fifth Atmoſpheres, the Sound *ſeem'd not to be augmented in proportion to what it was at the firſt and ſecond.*

However, it was obſerv'd, that at this fifth and laſt Injection, the Sound was very near as loud and ſenſible at the fifty Yards diſtance, as it was when the Bell was ſtruck in the open Air, without being inclos'd in the Receiver at all.

Now, the Reaſons of the Sound's not proportionably increaſing in ſo much greater Condenſations, I believe, may be theſe:

Firſt, *The Deficiencies of the injected Quantities of Air*. For the *Valve*, which ſhould have hinder'd the Return of the injected Air, might not perform its Office ſo exactly, or hold ſo tight as it ſhould have done; and by that means ſome Portions of Air might eſcape, and conſequently, the Quantities injected not be ſo great as was ſuppos'd: From whence it would be no great Wonder, that there ſhould be a Failure in the Proportion of the Increaſe and Propagation of the Sound.

Secondly, Tho' twenty five Compreſſions of the *Syphon* are equal to the Natural Content of the *Receiver*; yet when the Air becomes pretty ſtrongly condens'd (as 'tis by the Intruſion of four or five Atmoſpheres) the remaining Air at every ſtroke, which will lie between the Bottom of the *Embolus* and the *Valve*, though it be but little, yet is of the ſame Denſity at that time, as the Air in the Receiver: Which therefore, upon drawing up the *Embolus*, will extend it ſelf to ſuch a Space of the *Cylinder*, as it can fill up by expanding it ſelf into the State of Common Air; and is ſo much as this comes to, of what ſhould be injected at every ſtroke: Twenty five of which ſtrokes, as I ſaid before, are equal to the Natural Content of the Receiver.

And hence the Deficiencies of the real Quantities, which ſhould be injected by a certain Number of Strokes, may be very conſiderable; and to compute them, would be a Buſineſs of as much Difficulty.

Experiment II.

THE ſame Trial was made abroad in the open Fields, and with the ſame Succeſs as the former.

Upon ſhaking the Bell before any Air was injected, the Sound was but juſt audible at Thirty Yards diſtance.

When one Atmoſphere was injected, it was heard as diſtinctly at Sixty Yards diſtance, as before at Thirty.

Upon a Second Injection, the Bell might be heard at Ninety Yards diſtance. But after that, tho' near a Hundred Strokes of the *Forcer* were repeated, yet it could hardly be heard Twenty Yards further; which I attribute in great meaſure to the Reaſons before-mention'd.

The time when this Experiment was made, was early, about Five in the Morning, in the Month of *June*; the Weather very *Miſty*, and little or no Wind ſtirring. And the Silence requiſite for the nice making ſuch an Experiment, was by degrees interrupted by the Sounds of the Five a-clock Bells, and other Noiſes from the City: All which, in ſome meaſure, contributed to the Unſucceſsfulneſs of the latter part of the Experiment.

But this I hope ſome time or other to proſecute farther ; not deſpairing in the mean time, of contriving ſuch a *Gage*, as will ſhew the certain Quantities injected, without any Danger or Hazard in the Attempt.

Experiment III.

Concerning the Propagation of Sound in Rarefied Air.

HAving included a *Bell* in a *Receiver*, which was ſhaken to make it ſtrike, it was very obſervable that the Interpoſition of the Glaſs betwixt the *Ear* and the *Bell*, was a great Impediment to the Propagation of the Sound tho' it might be heard at a good diſtance from it. But the Air being gradually exhauſted, and ſeveral ſtops made, to ſhake the Bell at the ſeveral different degrees of Rarefaction ; I found that the Sound was remarkably diminiſh'd at each of thoſe ſtops. At laſt, when the Receiver was very well exhauſted, the Sound was ſo little, that the beſt Ears could but juſt diſtinguiſh it : It being like a ſmall ſhrill Note, heard at a mighty diſtance. As the Air was gradually admitted into the Receiver again, ſo the Sound gradually encreas'd creas'd ; this *Augmentation* in the more *denſe* Medium, anſwering by proportional degrees to the *Diminution* in the more *Rarefied* one. And when the Receiver was again replete with Air, the Sound ſeem'd ſomething more clear and diſtinguiſhible, than it did when the Bell was firſt included, before any of the Air had been drawn out.

The Obſervation therefore to be deduc'd from theſe Experiments, is this, *viz. That Sounds are augmented* in *Condens'd, and diminiſh'd* in *Rarefied Air :* Or, that That Undulating Motion in which Sound conſiſts, is propagated with more Facility and Advantage in *Condens'd* than in *Common* ; and in *Common*, than in *Rarefied Air*.

And from hence we may infer,

Corol. 1. That *the Diſtances at which the equally Strong Percuſſions of the ſame ſonorous Body ſhall be equally audible to the ſame Ear, in Condens'd, Common, and Rarefied Air, (or, which is the ſame thing, in Airs of different degrees of Denſity,) muſt be taken in ſome proportion to the Denſities of thoſe ſeveral Mediums, thro' which the Sound is thus propagated. And that therefore, were that proportion eſtabliſh'd by ſufficient Experiments ; from the Denſities given, the Diſtances might be inferr'd ; or from the Diſtances given, we might conclude the Denſities requiſite to make a Sound of a given Degree, to be equally audible at* *thoſe given Diſtances.* And therefore, were we to ſpeak of *the utmoſt Limits of Diſtance*, at which any given Sound is audible at all; 'tis plain that theſe Limits muſt be determin'd by the ſame Law of Proportion concerning the Denſities of the Mediums. Becauſe the *utmoſt Limits at which any given Sound is audible at all*, in any given Mediums, are likewiſe *the Diſtances, at which that ſame Sound is equally audible in thoſe Mediums.* For when a Sound is but juſt audible in any Mediums, 'tis then *equally* audible in thoſe Mediums.

Corol. 2. *The Diſtances at which the different or unequally-ſtrong Percuſſions of the ſame ſonorous Body ſhall be equally audible to the ſame Ear, in Mediums of different Denſities, muſt be taken, in ſome Proportion, compounded of the Strengths of the Percuſſions, and the Denſities of the Mediums.* And univerſally, to have Sounds *(cæteris paribus)* audible or diſtinguiſhible in any given Ratio, will require ſome Compoſition of the Proportions of *Diſtances*, *Denſities*, and *Forces of Percuſſion*.

Corol. 3. *Speaking ſtrictly, Sounds are not at all times equally audible to us here upon the Surface of the Earth.* I mean, the like Percuſſions of the ſame Sounding Bodies, are not at all times to be heard with the *ſame facility*, at the *ſame diſtances*. The Reaſon of which is ſufficiently manifeſt, ſince the *State of the*

Atmoſphere here about us, undergoes ſuch frequent Viciſſitudes; (and thoſe ſometimes very conſiderable ones too,) as to *Rarefaction* and *Condenſation.*

Corol. 4. *Sounds diminiſh or become leſs audible, as we aſcend upwards from the Surface of the Earth : And therefore in the upper Regions of the Atmoſphere; eſpecially in thoſe where the Planets revolve, ſonorous Bodies muſt be at a diſtance almoſt infinitely near, (that is, in contact with the Organ it ſelf;) or the Force with which they are ſtruck, almoſt infinitely great; for Sounds to be equally audible, with what they are here upon the Surface of the Earth.* The Reaſon of which is plain, from the prodigious Rarefaction of the Medium in thoſe Regions. A Globe of ſuch Air as we have here at the Surface, if plac'd at the height of a Semidiameter of the Earth, would expand it ſelf at ſuch a rate, as to fill all the Planetary Orbs as far as that of *Saturn*; nay, and a much greater Space than that. And that Medium, in which the Planets perform their Revolutions, is ſo fine and rare, as that its Reſiſtance is wholly imperceptible, though they have gone their Rounds in it for ſo many Ages. To what a degree of Rarefaction then does the Medium ariſe in thoſe ſublime Regions? And what perception ſhould we have there of ſuch Sounds, as are here propagated to our Organs with a great deal of Eaſe and Force? For, the ſtrokes of the ſounding Body being equally forcible, the diſtance of the Organ muſt be leſs, in a rarer Medium, in ſome proportion to that rarity; that the Sound may be equally audible as in a Denſer: And the Diſtances being the ſame, the Strength of the Percuſſions muſt be proportion'd to the Rarety of the Medium, in order to produce the ſame Effect: And conſequently, when the Rarefaction of the Medium is (as it is in thoſe Regions,) ſo vaſtly tranſcendent to what 'tis here on the Surface of the Earth; an Organ ſo made as ours is, muſt either approach almoſt infinitely nearer; or the Sonorous Body muſt be ſtruck with a Force almoſt infinitely greater; that a Sound may impreſs the Organ there, equally with what it does here. The crackling of Thorns in a Fire, would ſhake our Ear with a vaſtly-more conſiderable Force here, than the largeſt Canon, or the moſt dreadful Claps of Thunder, would do there; were either of them diſcharg'd at a much leſs diſtance from the Hearer than what we are now ſpeaking of.

The *Muſick of the Spheres* therefore is an Entertainment, which we ought to deſpair of ever hearing: And That *Conſort*, be it as Celeſtial as it will, yet wants a fit Medium (if *that were all that is wanting*,) to convey it to us. The old Philoſophers were much in the right, in ſaying, thoſe fine Sounds were never to be heard; and as much in the wrong, in laying down that for the Cauſe, that the Noiſe was too ſtrong and overwhelming to the Organ, for us to have any Perception of it.

Corol. 5. *The Diminution of Sounds in Aſcents or Elevations above the Surface of the Earth, will be in ſome Proportion to the Deſcents of the Mercury in the Barometer at thoſe Elevations.*

For were the Diminution of Sounds exactly in a *ſimple* or *direct* proportion to the Rarefaction or Expanſion of the Medium, at any Heights in the Atmoſphere; that Diminution would be exactly in a *reciprocal* Proportion to the Heights of the Mercury in the Barometer at thoſe Elevations; becauſe the Expanſions of the Air are found to be reciprocally as the Heights of the Mercury. And therefore if the Diminution of Sounds be in ſome *complicate direct* proportion of the Expanſions of the Medium, it will be alſo in ſome *complicate reciprocal* Proportion of the Heights of the Mercury in the Barometer. And conſequently, the Barometer might be made uſe of, to diſcover and determine the Diminution of Sounds in any Region of the Atmoſphere; provided it were well determined by Experiments beforehand, in what proportion Sounds diminiſh according to the Rarefaction.

An Account of an Experiment concerning the Reſilition *or* Rebounding *of Bodies in* Various Mediums.

I Provided a tall *Glaſs Receiver*, in the upper Part of which I had a Contrivance for the Lodgment of Four *Marbles* (ſuch as are generally ſold at the Shops) and from whence I could let them drop down on a *Plane* at pleaſure.

The Diſtance from the *Plane* to the Place where the Marbles were lodg'd (and conſequently the Space of their Deſcent) was about 13 Inches an half. And as to the Bulk of them, two of theſe Marbles weigh'd 59 Grains; and the other two, 63 Grains.

The Plane on which they were to fall, was a round flat Piece of ſolid Glaſs, about one Inch thick, and three Inches and an half over; the upper Surface of which was very well *ground* and *poliſh'd.* It was fix'd in a *Tin Frame*, contriv'd on purpoſe to keep its lower Surface from being contiguous to the *Plate* or *Leather* on which the Receiver was plac'd: The reaſon of which Contrivance was to prevent an Inconvenience which would otherwiſe ariſe; as ſhall be ſhewn by and by.

All things being thus provided, the Marbles *were dropped in Common Air*; that is, in the Air included in the Receiver.

After this, the Air was exhauſted, and *they were dropt in Vacuo*. And then an Atmoſphere of Air was injected, beſides the natural Content of the Receiver; and *they were let fall in that Condenſed Air*. I ſay, *one Atmoſphere*; for I did not dare to venture more, leſt the breaking of the Receiver (which would be a hazardous thing) ſhould have been the Conſequence of it.

Now, I found, that the Reſilition of the Marbles dropt in *Vacuo*, was ſomething more than that of thoſe in *Common Air:* And thoſe let fall in *Common Air*, had ſome Advantage in their Rebound, above thoſe let fall in *Air Condens'd*. The Rebound in *Vacuo* was about ten Inches and an half (which was more than three Fourths of their Deſcent.) In *Condens'd Air* it was about ten Inches.

Accordingly, in *Common Air*, we muſt count the Reſilition to be a *Mean* between the other two: For 'tis extreamly difficult to determine to a Nicety in a Motion ſo ſudden, and of ſo ſhort a Duration. But this, however, is certain, that there was a ſenſible difference between the Rebound of thoſe dropt in *Vacuo*, and thoſe in *Condens'd Air*. As for the *difference of the Weight of theſe Bodies*, I could not find that That made any diſcernible Alteration in their Reflection.

I would give one Caution here, which may ſerve to prevent thoſe, whoſe Curioſity may lead them to make Experiments, from falling into an Errour, which I my ſelf very narrowly eſcap'd.

The Glaſs, as I ſaid before, *was fixed in a Tin Frame, on purpoſe to keep the lower Surface of it from being contiguous to the Plate or Leather, on which the Receiver was plac'd* For when I firſt try'd theſe Experiments, I uſed a *Stone* Plane, laid careleſly *upon* the Leather, which cover'd the Plate on which the Receiver ſtood: And accordingly, the *Air being exhauſted*, the Marbles would not rebound ſo high by an Inch, as when the Experiment came to be made on the ſame Plane in *Common Air*. The reaſon of which was plainly this: That the Air being exhauſted, the Leather conſequently ſwell'd, and by that ſwelling rais'd the Plane which lay on it; and ſo cauſing it to lie more ſoft and hollow than when 'twas only in Common Air, by this means the Reſilition became leſs in *Vacuo*, than in *Common Air*; and the Event of the Experiment prov'd quite contrary both to what it ought to have been, to what was expected, and to what after came to paſs. For having fix'd the *Apparatus*, as before mention'd, all things ſucceeded then, both according to *Expectation*, and to *Philoſophical Theory*.

Corol. 1. *In any exact Computations therefore of the Reſilitions of Bodies, Account muſt be taken of the State of the Ambient Medium:* For the Rebounds of the ſame Body will not be the ſame, in all the various Conditions of that, as to Rarity and Denſity.

Corol. 2. *Here's a manifeſt Proof of the Air's Reſiſtance.*

I know, there are many Proofs beſides; but, I ſay, this Experiment alſo furniſhes one. For the difference in the Rebounds is no otherwiſe poſſible to be accounted for, ſince the Experiment may be rely'd on as made to a ſufficient degree of Nicety.

Some farther Experiments concerning the Electricity *and* Light, *produc'd from various Bodies by* Attrition.

EXPERIMENT I.

Being a farther Improvement of one made before, to the ſame purpoſe.

IT has been ſhewn in one of the foregoing Experiments, how Bodies included in a Glaſs, might be affected with a very ſenſible Motion, by the bare Approach of one's Finger near the Outſide. I have here ſomething to add to the Account of that *Surprizing Phænomenon*, which will render it more wonderful ſtill: And the *Appearance* in this Trial was ſo much the more Conſpicuous, by how much the *Apparatus* made uſe of was better contriv'd and adapted than in the former.

I obſerv'd then,

That when the Motion and Attrition of the Glaſs had been continued about two or three Minutes, and then ceas'd; the Threads ſeem'd to hang *Plate* VII. *Fig*. 6.

in great diſorder, and without any degree of Erection at all for ſome ſmall time. They continued in this poſture (as near as I could count) for about three or four *Seconds*, and *then they were extended every way towards the Circumference of the Glaſs*; and that with ſuch a ſtrength, that the Motion of the Glaſs alone would not very much affect 'em. But the ſtrangeſt thing of all was to ſee, that *a Motion might be impreſs'd upon them by the Approach of one's Finger, Hand, or any other Body, at more than three Inches diſtance from the outward Surface of the Glaſs, tho' the Threads themſelves did not touch the inward one.*

I obſerv'd further,

That every time the Motion of the Wheel and the Attrition of the Glaſs were repeated, the Threads might be mov'd by the Approach of one's Finger on the Outſide, at a ſtill greater diſtance.

Nay, I have found ſince, *that by blowing with ones Mouth only towards the Glaſs, at three or four Foot diſtance, the Threads would have a very conſiderable Motion given 'em.*

And when I have ſuddenly ſpread my Hands upon the upper and lower parts of the Globe, *there has been a violent Agitation of the Threads within, which has alſo laſted for ſome time.*

From theſe Obſervations we may gather,

Firſt, That the Cauſe of the Erection of the Threads (whatever it be,) tho' *certainly* excited by the Motion and Attrition of the Glaſs, *yet does not neceſſarily work its Effect* immediately, *upon that Motion and Attrition.*

For we ſee the Threads were quite looſe and motionleſs for three or four Seconds of time; and then they were extended, like ſo many *Radii*, towards the Circumference of the Glaſs.

'Tis worth enquiry here, Whether the Space of time between the *Ceſſation of the Motion*, and the *Beginning of the Erection of the Threads*, will be the ſame in *all Seaſons*, and in *all Conditions of the Ambient Air.* As alſo, *Whether the longer or ſhorter continuance of the Motion and Attrition of the Glaſs, before they ceaſe; does contribute any thing to the lengthning or ſhortning this Time of the unactive ſtate of the Threads, before they begin to be erected.*

Secondly, 'Tis manifeſt, *there's a Communication between the Medium without, and That within the Glaſs.*

This follows from the Motions and Tremblings of the Threads, upon the Approach of other Bodies poſited on the Outſide.

Thirdly, Not only a *Communication*, but a *Continuity*, of the Matter which occaſions the Motion of the Threads. The Progreſs of it ſeems to be in a ſtreight and direct track; in which the Matter is puſh'd by the *ſhorteſt Courſe*, from the *Approach'd Body* to the Threads that are ſhaken by it. And if the Threads are mov'd by influence of any Matter emitted from the Glaſs, it appears to be impoſſible to explain how they ſhould be ſo, and *at ſuch diſtances*, without a Continuity. So that the Caſe ſeems to be thus; That the Effluvia paſs along, as it were in ſo many *Phyſical Lines*, or *Rays*; and all the Parts that compoſe them, adhere and joyn to one-another, in ſuch manner, that when any of 'em are puſh'd, all in the ſame Line are affected by that Impulſe given to others.

And for this purpoſe the *following Obſervations* deſerve to be conſider'd.

Obſervat. 1. Having laid a piece of Leaf-Braſs between two pieces of Wood, about an Inch thick, and an Inch aſunder; I apply'd a well rubb'd Tube as near as the Wood would permit; but the Braſs receiv'd no manner of Motion. But as ſoon as the Wood was remov'd, it was attracted vigorouſly without any freſh Attrition of the Tube.

Obſerv. 2. When the Tube was well rubb'd, if a piece of Paper were immediately apply'd, ſo as to touch the upper Part of it; the Leaf-Braſs, ſcatter'd up and down upon the Table, would not be attracted at all, tho' the Tube were held very near: But upon removing the Paper, thoſe Bodies were put into ſenſibly brisk Motions.

Obſerv. 3. When a piece of Leaf-Braſs is hunted about a Room; it keeps its diſtance, according as the Effluvia are more or leſs vigorouſly emitted: Nor will it by any means ſink into the Sphere of the Effluvia, unleſs it meet a Body in its way; and then it will be *attracted* and *repell'd* ſeveral times, with a great ſwiftneſs.

Obſerv. 4. It may alſo be very properly urg'd upon this Account too; that in the Experiment *for producing Light by the Effluvia of the outward Glaſs, falling on the inward exhauſted Glaſs in Motion*; after the Motions were *ceas'd*, a Light might be produc'd on the inward Glaſs, by approaching ones Hand near the Surface of the outer One. Which ſeems convincingly to ſhew That Property of the Effluvia, I have been here ſpeaking of.

Plate III. *Fig.* 1.

Obſerv. 5. *When the Tube was fill'd with ſome other matter than Air, the Attractive Power of the Effluvia was conſiderably abated.*

Thus, when I had ſtopt up one end of it with a Cork, and fill'd its Cavity with dry *Writing Sand*; tho' the ſame Attrition was made as before, yet the Leaf-Braſs had no Motion given it till the Tube was brought within an Inch or thereabouts of it.

But if the Sand were ſuddenly ſhot out of the Tube, then it would attract the ſame Bodies at double or treble the forementioned Diſtance, without any new Attrition at all.

This laſt Experiment, compar'd with one before recited, makes way for a very conſiderable Obſervation: Which is this:

I have formerly ſhewn, *That when the Air contained in the Tube was exhauſted, the Attractive Power was quite loſt*, or very near ſo.

And here it appears, *That when the Tube was filled with a Heterogeneous Body, the attractive Power was exceedingly weakned.*

Now in both caſes there was an *Excluſion of the Air*; and in both Caſes the ſame kind of Effect follow'd, *viz.* the *Loſs of the Attractive Power.*

Only, where the Tube was *exhauſted*, the Air being *more perfectly excluded*, the Attraction was alſo more remarkably loſt, than when it *was fill'd with Sand*, by which the Air was excluded too, but *not ſo perfectly* as by the Exhauſtion.

Now I take the Reſult of theſe two Experiments in conjunction, *to be a ſignal Demonſtration of the Influence and Intereſt of the Air, in theſe Phænomena.* And if upon the filling the Tube with other ſorts of matter (than what I made this Trial with) *the Effect ſtill appears to be the ſame* ; it muſt then paſs for a Truth not to be diſputed. Tho' on the other hand, *if*, when the Tube was fill'd with other matter, *the Effect ſhould not anſwer in the ſame manner or degree*, but the Attractive Power ſhould be pretty ſtrong and vigorous : yet *this will be no concluſive Argument* againſt the Intereſt of the Air in thoſe particular Experiments, which I have here mention'd. For 'tis poſſible that other Matter may in one caſe yield that aſſiſtance towards the Production of the Effect, which ſome Action or Operation of the Air may afford in another different Caſe.

And I think it may ſtand for a very uſeful

Inquiry, *How far other ſorts of Matter, with which the Tube may be fill'd, will any ways influence the Attractive Force of the Effluvia, ſo as to make any ſenſible Alteration in it ?*

Experiment II.

Concerning the Electricity *of* Sealing-Wax.

I Fitted a Wooden Cylinder (of about ſour Inches Diameter, and three in Length) to an Axis, and plung'd it into melted Sealing-Wax, in which I kept it moving round till it had gotten a Coat of above ½ Inch thick, on its Surface. The Wax was of the beſt ſort I could procure; and the Quantity melted was 1½ *lib.* Having thus prepar'd the Cylinder, I plac'd it on the *Machine*, and gave it the uſual Motion and Attrition; which having been continued for ſome ſmall time, I held *Plate* VII *Fig.* 3. the *Hoop of Threads* (made uſe of in the *Experiment of the Glaſs Globe and Cylinder*) directly over it. The Effect was the ſame, as in thoſe Experiments. *For the Threads were directed by the Attraction, towards the Center of that Circle, in the Plane of which the Hoop* was placed. And while they remain'd thus directed, they would in like manner *fly the Approach of ones Finger.* And Leaf-Braſs would be *ſtrongly attracted* and *return'd*, or *carried about a Room*, by the Effluvia of the *Wax*, as I have elſewhere ſhewn it would be by thoſe of *Glaſs*. And the Effluvia of Wax likewiſe will be *ſenſibly felt* upon the back of ones Hand, if the Wax (after Attrition be moved to and fro, near it; juſt as thoſe of Glaſs may be. So that the *Electrical Qualities* of theſe two Bodies are the ſame, as to all the moſt *General Properties*. They differ only in *Degrees*; the Effluvia of *Glaſs* producing more Powerful Effects, than thoſe of *Wax*.

Concerning the Light *producible from* Sealing-Wax.

At *Night*, I gave the foremention'd Cylinder the ſame Motion I had given it in the *Day* (when I tried the Electricity of it,) and I applied ſome clean new *Flannel* to it; but there was very little or no Light at all produc'd by the Friction of *thoſe* two Bodies.

But when, inſtead of the Flannel, I applied *my naked Hand*, a *conſiderable Light appear'd*: the Properties and Circumſtances of which (as far as I have obſerv'd them) I ſhall comprehend in the following Particulars.

This Light was viſible only in that part, where the Attrition was made. The Light produc'd by the Attrition of the Glaſs Globe, was viſible by its odd Flaſhes, all over the

Surface of the Globe. It ſpread far beyond the part where the Attrition was made.

This Light depended moſt immediately upon the Motion ; and would continue no longer, than that continued. Some Lights produc'd by the Attrition of the Glaſs, have laſted for a while, even tho' the Motion has actually ceas'd.

None of this Luminous Matter would be communicated to ones Finger, when held near it. Whereas in the Lights produc'd from Glaſs it was otherwiſe.

This Light produc'd by the Friction of the Hand, on the Wax, in the open Air ; was hardly ſo conſiderable, as that produc'd by an Attrition made with Flannel, in Vacuo.

For that Light in *Vacuo*, was very diſcernible on each *Arm of the Braſs Spring*, that embrac'd the Flannel. And could the Attrition have been made with ones Hand in that *very rare Medium*, there's no doubt but the Light would have been ſtill much greater. So that in this Caſe there ſeems to be an agreement between the Lights produc'd from *Wax* and *Glaſs* ; viz. *That both appear to more Advantage in* Vacuo, *than in open Air* ; that is, in a very *weak* and *much rarefied* Medium, than in one of more *Force* and *Denſity*. And there's no Reaſon to believe, but all other Lights will agree in this property too: Nor is it at all ſtrange that it ſhould be ſo, conſidering the *fine and tender Nature* of the Effluvia, on which theſe Lights, produc'd by Attrition do depend.

Having thus ſhewn the Properties of this Light, produc'd by the Attrition of *Sealing-Wax*; I would ſubjoyn an Obſervation or two, which, I think, may deſerve notice.

Firſt, The Light produc'd by the Attrition of *Sealing-Wax* and *Woollen*, agrees exactly in one remarkable Property, with *that* which is produc'd by the Attrition of *Amber* and *Woollen*.

For *they* (*both of 'em*) *diſappear when the Attrition ceaſes.*

Secondly, This Light agrees in another Property, with *ſome* Light produc'd by the Attrition of *Glaſs* and *Woollen.*

For the Light of *Sealing-Wax is confin'd to that part only where the Attrition is made.* And ſo it is ſometimes in the Friction of *Glaſs* on *Woollen*: For tho' the Light does many times *ſpread all over*, yet at other times *it is limited only to that part which is rubb'd*: As was obſerv'd in one of the foregoing Experiments.

Thirdly, *This* Light, and *That* produc'd from *Glaſs*, agree in another conſiderable Property, tho' in very different Circumſtances of the Bodies themſelves.

For, *if a Glaſs Tube be* exhauſted *of its Air, the Light produc'd will not adhere to Bodies plac'd near it.* Nor will any Parts of the Light produc'd from *Sealing Wax*, in the *open Air*, faſten upon Bodies which are fairly expos'd to it, tho' brought very near.

So that the *ſame Property* which, in *one* Caſe, diſcovers it ſelf in the *open Air*, in the *other*, neceſſarily requires a *Vacuity*, in order to the Production of it.

So that the Effluvia of two different Bodies, (which otherwiſe do not agree in the ſame Property) may come to agree in the ſame Property, by the bare Alteration of an external Circumſtance, or by ſome Change in the State of the adjacent Bodies. As here, by the mere Subſtraction of Air from the Cavity of the Tube, the Luminous Effluvia of *Glaſs* (which otherwiſe had the Property of *adhering* to Bodies plac'd near) become now of that Nature, as *not to adhere*; wherein they agree exactly with thoſe of *Sealing-Wax*, provok'd by Attrition in the open Air.

Query. Suppoſing *Lac* and *Vermilion*, to be the Ingredients in the Compoſition of *Sealing-Wax*; whether the *Attractive Quality* be owing chiefly to the *Former* or the *Latter*? Which will be anſwer'd, by trying the Attractive Power of *equal Bulks or Maſſes* of the ſame Sealing-Wax, made up with *different Proportions* of theſe two Ingredients.

Ex. gr. Suppoſe I take any two Quantities of *Lac* and *Vermilion*, and with them make a *Spherical* or *Cylindrical* Body of *Sealing-Wax*: And then for a ſecond Compoſition, take either a greater or leſſer Quantity of *Lac* than before; and mix ſo much *Vermilion* with it, as will make a Spherical or Cylindrical Body, of the ſame Dimenſions exactly as the former.

If the *Lac* be *more*, and I find the Attraction of the ſecond Mixture *ſtronger* than that of the firſt; 'tis plain that the Attraction is principally owing to the *Lac*: But if the *Lac* be leſs, and the Attraction be ſtill ſtronger; 'tis plain that the Advantage of Attraction lies on the ſide of the *Vermilion*: Or *vice verſâ*.

And ſo with reſpect to any other *Factitious* Bodies, we may diſcover what Ingredients (and in what proportion) do principally conduce to this Effect.

And the ſame Enquiry may be uſefully made, with reſpect to the *Luminous*, as well as the *Attractive*, Quality.

And I ſhould think it no inconſiderable ſtep towards the Advancement of our Knowledge of the True Nature of Bodies, to be ſatisfied upon what Principles or Ingredients in their Compoſition, their *Light* and *Electricity* do mainly depend.

For this Point well ſettled with reſpect to *Factitious* Bodies; we might be enabled to judge more truly of the Cauſes of the like Effects in *Natural* Compoſitions.

Experiment III.

Concerning the Electricity, *&c. of* Sulphur *and* Roſin.

HAving provided two *Wooden Cylinders*, of the ſame Dimenſions as that mention'd in the former Experiment; I coated their Outſides, the one with *Sulphur*, and the other with *Colophony* or *Roſin* mix'd with *Brick-duſt*; which was added on purpoſe to bind the Roſin, and make it more hard.

Having given the *firſt* of theſe the uſual Motion and Attrition, I brought it near the *Hoop* fitted with *Threads*; and found, that the *Threads were attracted, and directed to its Center*; but not near ſo ſtrongly, as when the *Sealing-Wax* was uſed.

And this, upon ſeveral Trials, was ſtill much the ſame.

Then I try'd the latter, *viz.* the Cylinder coated with *Roſin*, and found, that *the Threads were drawn to the Center with more Force and Vigour*, even, than when the Experiment was made with *Sealing Wax*: But this is to be added,

ded, that the Roſin having been melted, was not quite cold at the time when this Trial was made.

This is the main of the Experiment; to which I muſt ſubjoin theſe following Obſervations.

Firſt, That when the Trial was repeated with theſe Bodies, the next Day, *the Electricity of both was ſo inconſiderable*, as *ſcarce to deſerve notice.*

Secondly, The *Roſin*, while *warm*, would attract *Leaf Braſs*, at the diſtance of an Inch or two, *without any Attrition at all.*

Thirdly, In both theſe Trials, the Threads would fly the Approach of one's Finger; but if *Sealing wax* or *Amber* were held near them, *tho' they were neither of them rubb'd*, *yet the Threads would have a ſtrong Tendency towards them.* A Phænomenon I never obſerv'd any thing of before; and which gives a very ſurprizing Inſtance of the *Attractive* and *Repulſive* Forces.

That the Threads ſhould be attracted by an Electrical Body, *while warm*, tho' the Attrition were ceas'd, (as for Example, by the Roſin, while it yet retained a Degree of Heat) this is no great Wonder: But that they ſhould be attracted ſted by ſuch a Body, in a State wherein that Body was perfectly free from any degree of Heat, and without any preceding Attrition to excite and rouze the *Effluvia*; this, I think, has ſomething very odd and peculiar in it. Nor do I think That centrifugal Motion of the Threads, upon the Approach of a Finger, leſs ſurprizing.

The Threads were altogether *in the ſame ſtate*, when each of theſe Bodies, that *produced theſe very different Effects*, were placed near them. And yet they were *repell'd* from ſome of 'em, and *attracted* by others. But now, before that the *Amber* and *Sealing Wax* on the one hand, or the Finger on the other, were brought near them, the Threads had been mov'd and *acted upon*, by thoſe *Electrical Bodies mentioned in the Experiment*. From whence theſe Two Queries:

Firſt, Does that *previous Motion and Excitation of the Threads*, any way *cauſe* or *contribute* to this ſo very different an Effect, of their *flying from one Body*, *and ſtrongly tending to another?*

Or, *Secondly*, Is the Reaſon of this Phænomenon to be entirely deduc'd *from the Natures of the Bodies themſelves, to which the Threads were expos'd*; ſo that by virtue of *ſome Law* or other, unknown to us, the Threads ſhould *tend towards This Body*, and *fly the Approach of That?*

Which of the two is the true Cauſe, I muſt leave at preſent to farther Enquiry: And I think alſo, it will not be very difficult.

Thus much for the *Electricity* of theſe Bodies. As to their *Luminous Quality*, I have but little to ſay.

Upon an Attrition of the *Roſin* in the *Dark*, *I could find no Light at all.*

And but very little from the *Sulphur:* And that not by a Friction made with my open Hand neither; but by holding the Ends of my Nails very hard upon it, while it was in motion.

And therefore, *it either contains but a very ſmall Portion of Luminous Matter in it*; or elſe *That Matter is ſtrongly retain'd within the Body of it:* So that the ordinary Degrees of Attrition are not ſufficient to bring it forth.

I am apt to believe, that the *Latter* is more the true Reaſon, why ſo ſmall a Quantity of Light is producible from Sulphur, than the *Former*. For *Action* and *Re-action are equal in all Bodies*.

Now, as Light acts more upon *Sulphur*, and *Sulphureous* Bodies, than it does upon any others; ſo, reciprocally, Sulphur acts more upon *Light*. And therefore, *it being more ſtrongly held* in the Body of the Sulphur, by virtue of that *Law*, the *Emiſſion of it is much more difficult.*

For *ſuch a Degree of Attrition, the* Momentum *whereof exceeds the* Momentum *of the Attraction of the contained Luminous Matter by the Body which contains it ; is neceſſary to educe Light, or Luminous Matter, out of that Body.*

And therefore Bodies which do with equal Facilities emit their Light, ſhould ſeem to have equal attractive Forces on that Matter.

And *Univerſally*, The Attractions ſhould be proportional to the Forces of Attrition, all other Circumſtances being alike.

I try'd whether *Sulphur* would emit any Light, by an Attrition in *Vacuo :* But with all my Endeavours, *I could find none.*

Now, there was a very vigorous Light produc'd by an Attrition of *Sealing-Wax* in *Vacuo*; more conſiderable than that produc'd from the ſame Body in the Open Air. Whereas the Effects of *Sulphur* were juſt the contrary : There was a ſmall Light, produc'd with much Labour, in the *open Air* ; and none at all in *Vacuo*.

Query 1. Does the *Abſence of the circumjacent Medium* any ways contribute *to the more ſtrong Retention* of the Luminous Matter in the Body of the *Sulphur?* If ſo; it *has an Influence to produce the contrary Effect* in the caſe of the *Sealing Wax*, where a brisk Light appear'd when the Air was withdrawn.

Or, 2. Are the Effluvia of the Sulphur indeed emitted in *Vacuo*, as well as in the open Air; but not ſenſibly *Luminous* in that State of the *Ambient Medium*, that is, not *Viſible?*

Experiment IV.

Concerning ſome very Uncommon Effects *of the* Effluvia *of Sealing-Wax.*

IN that Experiment where the Threads are included in a Glaſs Globe, and, upon the Attrition of it, *point every way from the Center to the Circumference*, it was obſerv'd, that, in that State, a Motion might be given to the Threads, by the Approach of ones Hand near the Outſide.

And this odd Appearance, we know, is to be attributed to the Effluvia of the Glaſs, excited by the Attrition. For by ſome things formerly mention'd, it appears, they are endowed with a Quality, which renders them capable of producing ſuch Effects.

But I find, that *the Effluvia of other Bodies held without the Globe, will alſo perform the ſame thing*. Tho' the Threads *are included there*, and *the Globe has no Motion nor Attrition at all given it*; yet if *another* Electrical Body be plac'd near, they will *move after a very ſtrange and ſurprizing manner*.

For they did ſo when I had rubb'd *Sealing-Wax* at the diſtance of three or four Inches from the Globe. Alſo *Amber*, or a *Glaſs Tube*, would produce the ſame Effect.

Leaf-Braſs, cover'd cloſe with a flat bottom'd Glaſs upon a Table; would have a brisk Motion given it, by holding the rubb'd Sealing-Wax over it. And *one ſingle Attrition* of the Wax, would *be ſufficient to keep thoſe included little Bodies* ſtirring for a conſiderable time. Nay, they have *continued their Motion* after the Wax has been *taken away.*

This ſhews the Penetration, Subtily, and very great Activity of the Effluvia (at leaſt of theſe) *Electrical Bodies.*

But 'tis to be noted here,

Firſt, That *this Experiment will not always ſucceed.* Sometimes not at all; much leſs in that degree, I have here related. And the Reaſon of this I take to be from a more Humid Temper of the Air, in which ſtate ſome little moiſture was probably condens'd upon the Surface of the Glaſs; and enough to be ſure, might eaſily be, to obſtruct the Paſſage of Bodies ſo fine and ſubtile as theſe Effluvia.

Secondly, This *Inconvenience may be remedied, when it does happen.* For if the Glaſs be plac'd a while in the *Sun-ſhine*, or a little warm'd by the *Fire*, or well rubb'd by a warm dry *Linnen Cloth*; then the Leaf-Braſs, if the rubb'd Wax be held over it, will be put into as brisk Motions as before.

Thirdly, This *warming or rubbing of the Glaſs, ſeems not only to clear it of the moiſture, that might be condens'd on its Surface; but alſo, by actuating the parts of the Glaſs themſelves, and perhaps raiſing ſome little Quantity of Effluvia from it, encreaſes the Force of thoſe of the Sealing-Wax, and renders their action on the included little Bodies more conſiderable.*

This I conclude from hence, *viz.* That when I had us'd any of the forementioned Methods, to clear the Glaſs from any thing of Soil or Moiſture it might have contracted; I found I could *give a Motion to the Leaf-Braſs, only by rubbing my Finger on the Outſide of the Glaſs, without any Aſſiſtance from the Wax.* But yet when the rubb'd Wax was held over it, *the motion of the included Bodies would be much more brisk.*

However, when the Air is *warm* and *dry*, I never found any occaſion to do any thing to help forward the Action of the Effluvia; their Paſſage being then ſufficiently clear; and the Bodies within, ſhewing by their various agitations how much they lie expos'd to their Power.

An Account of the Succeſs of an Attempt to keep ſeveral Atmoſpheres of Air condens'd in the ſpace of one, for a conſiderable Time.

I Took a very *ſtrong thick Flint-Bottle*, which I had procur'd to be made on purpoſe for this Experiment; into which I injected with my *Syringe* between four and five Atmoſpheres of Air; as an included *Gage*, of about 4½ Inches in Length, plainly enough ſhew'd. For, the Mercury riſing up ſo far, as to fill about ⅚ of the whole Gage, conſequently compreſs'd the Air in the upper part of it, into nearly ⅙ part of the Space it poſſeſs'd before. This Air continued in that ſtate of violent Condenſation from *March* the 30th, till about the 7th of *Auguſt* following, At which time happening to look on it (as I uſually did once in four or five Days) I found that the *impriſon'd Element had made its eſcape.* Nor was I at any loſs for the Cauſe hereof, when I conſider'd the *Intemperate heat of the Weather* for ſome time before.

For one day eſpecially, I obſerv'd that the *Spirit in the Thermometer* had aſcended 120 Degrees above the *Freezing Point*. This hot ſtate of the Ambient Medium, was ſufficient to produce the formentioned Effect; and to render the *Cement*, by which the Braſs-Cap of the Bottle was faſten'd, (*even tho' it was preſerv'd*, for the greater ſecurity, *under Water*,) to make it, I ſay, ſo ſoſt and yielding, as not to be able to reſiſt the Efforts of the mighty Spring of the inclos'd Air.

By this means, all thoſe parts of it, whoſe Springs preſerv'd their *Tone*, readily exerted themſelves, and got away out of the Bottle, leaving others behind them, which were not able to unbend, and conſequently to gain themſelves Liberty. For I found that the *Mercury* in the *Gage*, continued ſtill about three Fourths of an Inch above the Surface of that wherein the open end of the Gage was immers'd; by which it appear'd, that the Air in the upper part of the Gage ſtill remain'd compreſs'd into a ſpace, which was about one Sixth part leſs than what the ſame bulk took up before the Injection.

But what deſerves moſt particular Conſideration is, the *Mercury ſtill kept the ſame height, after its Surface in the Bottle was expos'd to the open Air*. So that thoſe remaining parts of the Air inclos'd within the Gage, though they *had all the Scope and Freedom poſſible to expand themſelves, yet did not do it*; and thereſore were ſome way or other *render'd uncapable* of ſo doing.

Had they been as able as the others, which went off before; they had likewiſe gone off too. So that their long detention in that violent ſtate, muſt needs have made them unable to unwind themſelves, ſo far as was neceſſary to their own diſcharge.

And had not the forementioned Accident happened, but they had continued in the Condition they were in at the firſt injection; *there's no doubt but the diſorder they ſuffer'd* would have been ſtill greater, and their *incapacity of Reſtitution* conſiderably more.

Thus much for the Experiment it ſelf.

And tho' the Succeſs of it was not ſo perfect as might be wiſh'd for, yet 'tis ſufficient to inform us,

That *Air by long and violent Compreſſions, may* (to all Appearance) *be depriv'd of much of its Elaſtick Power.*

That the Self-reſtoring Quality of thoſe *fine Springs* (which in many inſtances produce ſuch wonderful Effects,) is ſo far impair'd by their being *held bent* for a long time together, that afterwards they do not ſenſibly exert themſelves, tho' plac'd in the moſt *favourable and likely Circumſtances* for ſo doing.

Thus we ſee, that That Portion of Air which was left in the Gage, was not able to depreſs the Mercury, tho' the Surface of the Mercury in the Bottle was now expos'd to the open Air. The *injur'd Springs* could not recover themſelves to their former *Tone* and *Temper*; but continu'd in a ſluggiſh Reſt upon the *Mercury*, even after way was made for their *free and eaſie Expanſion.*

Now, if this was the Effect of the Condenſation of *leſs than five Atmoſpheres* of Air; *the greateſt part of which made its eſcape*; and the *longeſt time* that any part of it was held in this ſtate of Violence, *was little more than four Months*; What would it be if *nine or ten Atmoſpheres* were crouded into the room of one, and continu'd in that condition for *as many Years as the other did Months?* Would not this Air loſe its Elaſticity much more than the former? *Would its tender Springs be able to unwind themſelves much, after ſo long and powerful a Reſtraint?*

Would not the Mercury *keep its Height and Station in the* Gage, *notwithſtanding that the Veſſel containing Air ſo compreſs'd as this, ſhould afterwards come to be expos'd to the open Element?*

What kind of Liquid would ſo many Atmoſpheres of condens'd unelaſtick *Air, compoſe?*

Would not Terreſtrial Animals be ſuffocated in it, as they would be in Water or other Fluids?

Would not Fire *in like manner be quickly extinguiſh'd, and perhaps with ſome noiſe and hiſſing, if put into ſuch a Fluid as this?*

Might not light Bodies (ſuch as thin Glaſs Bubbles) *float upon ſuch a Medium; ſuppoſing them not above five or ſix times ſpecifically heavier than common Air?*

Is it impoſſible that Air by this means ſhould become a Viſible, Palpable *Fluid; and be ſubject to ſome of the ſame Managements that other Fluids are?*

What would be the conſequence of the Action of an intenſe Heat upon Air thus compreſs'd and depriv'd of all its Spring? Would it *rarefie*, and at laſt *recover its Elaſticity again, by the Changes poſſible to be wrought* by the long continu'd Action of Fire? Or, would its Parts be only violently hurried about, as thoſe of other Liquids are by the like cauſe, which afterwards ſettle and compoſe themſelves again?

An Experiment concerning the Production of Light in an exhauſted Glaſs, lin'd withinſide with Sealing-Wax, *upon an Attrition made without.*

HAving procur'd a Glaſs *Globe* of about ſix Inches Diameter, I put into it a convenient Quantity of broken *Sealing-Wax*, and held it over a *moderate* Fire till the Wax was melted.

Then turning the Globe about, that the Wax might ſlip from one place to another; it had quickly got a pretty thick Lining, on more than half its Inſide: But 'tis to be obſerv'd, that it was not in all places equally thick, it being impoſſible to manage the melted Wax in ſuch a manner as to make it ſo.

Having done thus, I plac'd the Globe in *a fit* Poſture, and *left it till it was perfectly cold*; and then having fix'd the *Braſs work* to it, I *exhauſted it of its Air.*

It was, immediately upon this, applied to the *Machine, repreſented in* Plate VII; *where the manner of giving motion to it is ſo obvious, as needs no Deſcription:* And then making an Attrition with my Hand, I obſerv'd the following *ſurprizing Phænomenon*, which *the Evening*

ing (the proper time for ſuch Obſervations) permitted me to do with great advantage.

My Hand was no ſooner applied to *that part of the Globe which was lin'd with the* Sealing-Wax, but *I ſaw the ſhape and figure of all the Parts of my Hand* (which touch'd the convex Surface of the Glaſs) *diſtinctly and perfectly upon the concave* Superficies *of the Wax within.*

When the Glaſs alone, without any ſuch Lining on the Inſide, is made uſe of; 'tis obvious to any one (who has ſeen, or does but conſider the Experiment) how plainly a Hand muſt be ſeen, which is plac'd on the convex Surface of a Globe all over enlighten'd with a ſtrong-flaſhing Light.

And perhaps it may ſeem ſtrange, if I ſhould ſay, that *the Appearance was now as plain and viſible as then*, *notwithſtanding the Interpoſition of the thick Body of Wax.* 'Twas as if there had been only pure Glaſs, and no Wax in the way; or, as if the Glaſs had been away, and the Wax were tranſparent.

This *Lining*, where it was ſpread the *thinneſt*, would but juſt allow the ſight of a Candle through it in the *dark*. But in ſome places it was at leaſt one eighth of an Inch thick. And yet even in thoſe parts, *the Light and Figure were as diſtinguiſhible* as any where elſe. Nay, tho' ſome parts of the Sealing-Wax did not adhere ſo cloſe to the Glaſs as others, *yet the Light appear'd on theſe, juſt as on the reſt.*

This Light produc'd, was *not diſcernible at all thro' the Body of the Wax*; but was to be ſeen by looking thro' the other Parts, where the Glaſs was free and tranſparent.

The *Colour*, and other Properties of it, reſembled thoſe of *the Lights produc'd from pure Glaſs*; except in this one Inſtance, *That upon the Admiſſion of a ſmall quantity of Air into the Globe, the Light wholly diſappear'd in that part cover'd with the* Sealing-Wax, and not in the other.

When all the Air was let in, the Hoop of Threads being held over the Glaſs, *the Threads were attracted at greater diſtances, by that part which was coated with the Wax*, than by the other. But *even then*, *when all the* Air *was exhauſted*, the Wax would attract Bodies placed near the Outſide of the Glaſs: For in this caſe I found, *the Threads had their Central Direction*, tho' *not ſo vigorouſly*, as when all the Air was let in.

But this is further remarkable too, with reſpect to that State of the Air's Abſence, *viz.* that *the Threads would not be attracted, if held over that part of the Glaſs which had no Lining of* Wax *on the Inſide*: Whereas if they were brought within the Sphere of the *Effluvia of Wax*, they would direct themſelves towards it.

So that in the Courſe of this Experiment, there are theſe following things to be taken notice of:

Firſt, *A diſtinct and lively Repreſentation of the Shape and Form of an Object, upon a ſolid opaque Body, to which that Object was not immediately applied; and this by the Light produc'd upon the Attrition of another Body (ſolid,* tho' not *opaque) to which that Object was immediately applied.*

A Man would have been thought the Author of a very ſtrange *Paradox*, that ſhould have aſſerted this, and at the ſame time conceal'd the Experiment; which ſhews how and which way 'twas done.

Or, ſhould it have been propos'd by way of *Problem*, thus: *To repreſent the Figure of an Object (plac'd behind an opaque Body) upon the contrary ſide of that opaque Body*; and *this without the help of Optick Glaſſes, or any Foreign Adventitious Lights:* Perhaps the Solution might have been thought impoſſible; or, it may be, the very Terms of the Problem it ſelf, abſurd and contradictory.

For, the Body on which the Figure is to be ſeen, muſt be an *opaque* one, (by the Hypotheſis;) and the Object it ſelf plac'd on the contrary ſide to that which it is ſeen on: So that either the Light muſt *be tranſmitted* thro' this Body, and then *'tis not opaque*, which is contrary to the Hypotheſis; or elſe the Light muſt *not be tranſmitted*, and then *no Figure could be ſeen:* For all diſtributions of Light by *Optical Artifices*, are excluded by the firſt Suppoſition.

But we ſee, this is not only poſſible, but alſo plain Matter of Fact.

From whence, I think, it may be uſeful to obſerve, *That many odd Effects and Appearances, which we may argue very plauſibly to our ſelves, againſt the Poſſibility of; and ſeem to find downright Abſurdities and Contradictions in; may yet be brought about by the genuine Forces of Nature, acting in convenient Circumſtances, upon proper and ſuitable Bodies.*

And from hence; That we do not, upon ſuch occaſions, proceed to conclude too peremptorily, what may or may not be done; and think, that every Difficulty or *Apparent* Impoſſibility to us, is a real one to Nature it ſelf.

Secondly, *The uniform Clarity and Perſpicuity of the Figure repreſented, thro' all the Parts of the opaque Body,* (*viz.* the Wax,) *on which it was ſeen; being as viſible in the thickeſt and groſſeſt, as in the fineſt and thinneſt parts of it; and on thoſe which lay out farther from the Glaſs, as on thoſe which adher'd more cloſely to it.*

Thirdly, *A total Diſappearance of the Light in all that part cover'd with the Wax, upon the Admiſſion of a ſmall Quantity of Air; and its Continuance in the other parts of the Glaſs* at the ſame time.

Both theſe laſt-recited Heads do alſo furniſh ſomething that looks pretty ſtrange.

Here's a Figure tranſmitted thro' the moſt Denſe and Compact parts of an opaque Body, with the ſame Facility and Advantage to the Eye, that it is thro' thoſe which ſhould ſeem the moſt eaſily pervious to the radiant Matter which is to form the Repreſentation.

Again: Here's a *notable Diſtinction obſervable in the Lights produc'd.* They were ſuch, that one and the ſame Cauſe deſtroy'd the one, and left the other untouch'd. The Air ſwept away all which aroſe from the *parts lin'd with* the *Wax*; whilſt the other Regions of the Glaſs preſerv'd their Light without any diminution.

Fourthly, *The more ſtrong and vigorous Attraction from that part of the Glaſs lin'd with the Wax, than from the other:* Which was manifeſt by the extent of the Attractive Power, from thence to greater diſtances than what the other would reach to.

Fifthly, *The Attraction and Central Direction of the Threads to the Wax, even while the Globe was exhauſted of its Air.*

This anſwers to a like Phænomenon of the *Load-ſtone*; whoſe *Effluvia* will work their Effect, even when the *Stone* it ſelf is plac'd *in Vacuo.* So here the Threads were puſh'd towards the Wax, when at the ſame time it was included in a Glaſs, whoſe Air was drawn out. But then here's this difference, that *the Threads were leſs vigorouſly drawn in this State, than when all the Air was let in*; whereas all *Magnetick Attractions* are (*at the leaſt*) equally ſtrong in *Vacuo*, as in the *open Air.*

Sixthly, *The Limitation of the Sphere of Attraction to that particular part of the Globe, which had the* Wax *on the Inſide*, (during that ſtate of the Air's abſence.)

Theſe things thus obſerv'd; we may now reaſon a little upon 'em, in the *modeſt way of Enquiries.*

1. *May not one Body* attract (and as it were imbibe) *the Effluvia of another contiguous Body; eſpecially when Motion and Warmth have made an eaſie paſſage for ſuch Effluvia into the Interſtices of that Body, whoſe attractive Power tends to fetch them thither?*

2. *Might not* (therefore) *the* Sealing-Wax, *by vertue of that Law, incorporate with it ſelf the Luminous Effluvia emitted from the contiguous Glaſs?* Glaſs gives a free paſſage to the Effluvia of Sealing-Wax: May not Sealing-Wax (on the other ſide) as freely admit the Effluvia of Glaſs?

3. Suppoſing the Body of the Sealing Wax thus charg'd and repleniſh'd with the Luminous Effluvia of the Glaſs; Would it not in that ſtate appear Luminous it ſelf? Do not all Bodies that ſhine, do ſo by virtue of Lucid Matter lodg'd in them; and in ſome degree, more or leſs forcibly darted from 'em? Why ſhould not Wax, every where replete with ſhining Corpuſcles, appear ſhining, as well as *Wood* charg'd with Fiery Parts, gives us the Senſation of a Burning Coal; or *Smoke* throughly heated, that of a lively Flame?

4. *What is it to be* pellucid, *but to* tranſmit Light receiv'd? *And does not the* Wax *thus tranſmit the Luminous Matter attracted and imbib'd from the Glaſs?*

5. *Has not the* Wax (therefore) *in this ſtate a ſort of Tranſparency?* I ſay, *in this ſtate:* For *the Property is limited to the preſent Circumſtances* of theſe Bodies concern'd in the Experiment?

During the Attrition, there is an Eruption of Luminous Effluvia from the Body of the Glaſs.

Does not the Attraction take place, *as ſoon as the Matter to be attracted is furniſhed* by the contiguous Glaſs? Is not the Wax ſaturated with Light as ſoon as the Attraction commences? And when the Wax is ſaturated with

Light, does it not then appear luminous? (that is, does it not communicate ſome Parts of the *Light revived*, to the circumjacent Medium?)

6. Since therefore the Sealing-Wax in this ſtate, is not to be conſider'd purely as an *opaque Body*, which oppoſes the Tranſmiſſion of Light (as it really is in all other Circumſtances) but as a Body every where pervious to the Lucid Matter emitted from the neighbouring Glaſs; *May we not from hence conceive, how the Figure of an Object placed on the one ſide, may be repreſented on the contrary ſide thereof*, (namely, that ſide which is turn'd towards the Eye of the Spectator? Why ſhould I not as well ſee my Hand plac'd on the Glaſs, whilſt the Wax is *thus open to the Luminous Effluvia*; as ſee it when I place it behind any ordinary tranſparent Body whatſoever?

In a word; 'tis plain matter of Fact, that *the Figure of the Hand is ſeen on the contrary ſide of the* Wax. And 'tis demonſtrable from the very Circumſtances of the Experiment, that *That Figure is not form'd there by any of the common Ways of picturing Objects by* Reflected *or* Refracted *Light*.

The Figure therefore is tranſmitted through the Body of the Sealing-Wax. But no Species or Picture can be tranſmitted thro' an Opaque Body, while it continues to be Opaque; that is, while it continues *impervious to the Rays of Light*.

Light. Therefore the Wax muſt, *at that time*, be in the *contrary ſtate*; that is, *pervious* to the Luminous Matter.

This Luminous Matter is originally emitted from the Glaſs in the Act of Attrition; but how it ſhould paſs from thence into the Body of the Wax, without an Attractive Force bringing it thither, I cannot, at preſent, tell how to conceive.

7. *Qu.* Whether the *Figure* was not as Diſtinctly form'd on the thickeſt, as on the *thinneſt Parts of the Wax*, *upon account of the* Quantity of Luminous Effluvia *every where attracted in proportion to the Quantity of* attracting *Matter*? And whether it was not upon account of the very ſmall (Comparative) Difference between the *Diſtances of the fartheſt and neareſt Parts of the* Wax, *with reſpect to the Glaſs*; *that the Luminous Matter was pretty equally drawn to both*, and *ſo the Appearance became* (to Senſe) *equally diſtinct on both*? Or, that the *Vibratory Motion* of the Effluvia at their Eruption from the Glaſs, might bring them as well within the Attractive Sphere of the remoteſt, as the neareſt Parts of the Wax?

8. *Is not the more ſtrong and vigorous Attraction from that Part of the Glaſs lin'd with the* Wax, *cauſed by the* united attractive Forces *of the* Glaſs *and* Wax?

An Account of ſeveral Experiments about the Aſcent of Liquids, between the nearly contiguous Surfaces of Bodies.

WHenever we give *Natural Cauſes* an Opportunity of exerting themſelves in the *ſame* or *ſimilar* Circumſtances, we have reaſon to expect the *ſame* or *ſimilar* Effects. If any Phænomenon be the Reſult of ſuch a Principle or Power in Nature, upon *ſuch or ſuch an Application or Diſpoſition* of External Matter; then, when the like Diſpoſition is made again, there's little doubt of the Appearance of the ſame Phænomenon. Some Effects indeed, there are, plainly reſtrain'd and conſign'd to ſome particular Qualities of Matter; as the Phænomena of Light and Electricity (before diſcours'd of;) which don't ſucceed in all Bodies alike. Others depending upon a far more General and Comprehenſive Cauſe, require no more, in order to their Appearance, than Fit Circumſtances, or a convenient Diſpoſition of Bodies, with reſpect to one another; and ſo, things being brought within the Sphere of that Cauſe on which ſuch Effects depend, they are immediately produc'd of courſe, by ſome Univerſal Eſtabliſh'd Law of Nature. Of this latter ſort, if I do not greatly miſtake, are thoſe *Phænomena* which we have now under conſideration.

The Experiments made upon the Aſcent of Liquors in ſmall Tubes, gave me an occaſion to think, what Varieties might occur upon the making the Experiment, after a manner different from what before had been us'd: And what Succeſs I have had in theſe Trials, I have here given a large and particular account of, under the following Heads. In all which the Philoſophical Reader will diſcover an exact Uniformity of Appearances and Effects, conſequent upon the Similar Circumſtances and Conditions of External Bodies.

Here were no *ſmall Tubes* made uſe of in any of theſe Experiments: But when Bodies were placed together in ſuch a manner, that ſomething equivalent to *ſmall Tubes* would neceſſarily reſult from their very *Poſition*, with reſpect to one another; then the ſame thing would always come to paſs, that would have done, had *common ſmall Tubes* themſelves been made uſe of.

Experiment I.

Of the Aſcent of Liquor between two Glaſs Planes *in the* open Air.

I Procur'd a couple of *Glaſs Planes,* which were part of a broken Looking Glaſs, being about ſeven Inches long, and one Inch and an half in breadth.

Now though theſe, when clapp'd together, were very cloſe, as ſeeming to *touch in many Parts,* yet *when they came to be immers'd* in a Liquid, it *would aſcend between them:* As was manifeſt upon their Separation, when they were *found actually wet on all their Parts.* But this Liquor being ſo *thin* and *colourleſs,* the Aſcent of it between the Planes was not ſo *eaſily diſcernible.*

Wherefore, to make it more obvious, I put a *ſmall Piece of Paper* on each Corner, by which means they were ſeparated by *an Interval equal to the Thickneſs of the Paper,* when they came to be apply'd to one another.

This done, I *plung'd one End* of them under the Surface of a *ſtrongly-tinged Liquor;* upon which it began immediately to aſcend,

but

but *not with that Swiftneſs as in ſmall Tubes*: However, the Motion of it was very odd, being ſometimes *higher in one part than in another*, and ſhooting out very pleaſantly into diverſe *Branches*; which it continu'd to do, till it had *reach'd its greateſt Height*.

But the Height of its Aſcent *varied according to the diſtance of the Planes*. For if inſtead of *one Piece* of Paper on each Corner, *two* were laid there, the Liquor would *not mount ſo high* in the latter caſe, as in the former, when the Planes were ſeparated only by *a ſingle Paper*. And then, if the Planes were *any ways declin'd*, the Liquor would ſtill ſpread it ſelf farther and farther, in *proportion to the Degree of Declination.*

And, upon ſeveral Trials, this all ſucceeded much after the ſame manner.

Experiment II.

The ſame in Vacuo.

BEing willing to try the Aſcent of the Liquid between the Glaſs Planes in an *exhauſted Receiver*, as well as in the *open Air*; I fix'd the Planes ſo to a *Braſs Wire* (which paſs'd thro' the Cover of a *Receiver*) that I could make 'em deſcend at pleaſure. In this manner I convey'd them into the *Receiver*, together with a *Diſh of tinged Liquor*; which having plac'd on the *Pump*, the *Gage* in a little time ſhew'd the Air to be pretty nicely drawn out. Then I plung'd the Plates (ſeparated by Pieces of *Thin Paper*, as before) into the Liquor; *which aroſe between them, as in the Open Air.* Nor was there any other difference than only this; *That there appear'd more Intervals or Spaces between the Branchings of the aſcending Liquid*, than when 'twas try'd in the *open Air.*

However, *when the Air came to be let in again, thoſe Spaces were fill'd up with Liquid*; which was now an *entire Body*, without interruption.

Experiment III.

The Aſcent of Liquids, between Marble *and* Braſs Planes, *in the* open Air.

I Procur'd a Pair of *Marble Planes*, ground as true as the Workman could poſſibly make them: Theſe I joyn'd together *dry*, and *without any thing betwixt 'em;* Which having done, I *immerſed* the lower Edge of 'em about ¼ of an Inch *below the Surface of the Water*, and held 'em there for ſome Minutes: Then taking 'em out, I found I could not eaſily part 'em, without *ſliding* 'em one off from the other. But having ſeparated them that way, I preſently found how far the Water had inſinuated it ſelf betwixt them.

This Aſcent of the Liquor *I found, upon various Trials, to be different;* but always obſerv'd, that *when I had newly rubb'd the Planes over with Wood-aſhes, the Water would aſcend the higheſt.*

After this, I made uſe of a Pair of *round Braſs Planes;* which having order'd as before, *the Succeſs was very agreeable to what it was in the former Caſe.*

And there's little reaſon to doubt, but the ſame thing would happen, if any other ſort of Bodies were us'd, whoſe Surfaces are very *plain* and *ſmooth*, and poſited ſo, as to be *nearly contiguous* to each other.

Experiment IV.

The Aſcent *of Liquors between two* round Glaſs Planes, *in the* open Air.

I Laid theſe round Planes one on the other, without any thing to ſeparate them; and having plunged the round Edge juſt under the Surface of the tinged Liquor, I obſerv'd, *That it immediately ſpread it ſelf thro' the whole Surface of them, and reached the extream Parts.*

In the *other* Caſes (before mention'd) a *ſtreight flat* Edge was applied to the Liquid; but *here* only a *circular* one; ſo that fewer Parts of the Glaſs were dipp'd in this Trial, than in the former, where *ſquare* and *oblong* Planes were uſed. Notwithſtanding which difference, the Water mounted upwards, and that in as *little time* too, as in the former Experiments.

Experiment V.

The Aſcent of Water thro' a Tube fill'd with Aſhes, *in the* open Air.

I Took a Glaſs Tube, whoſe Length was 32 Inches, and the Diameter of its Cavity near three Fourths of an Inch. To one end of this Tube I ty'd a *piece of Linnen Cloth*, and then fill'd it with *Aſhes*, which had been *ſifted* thro' a pretty *fine Searſe*. As I put in the Aſhes by *ſmall Quantities* at a time, I ramm'd them down ſtrongly with a *Rammer*, whoſe *Baſis* was very little leſs than the *Bore* of the Tube; by which means *I crouded them together as cloſe as was poſſible.* When the Tube was full, I ty'd over that end of it (by the *Neck*) a *thin limber Bladder*, (which I freed from all its included Air) in order to receive that Air, which I expected would be *forc'd thro' the Aſhes upon the Aſcent of the Water.* This done, I plung'd that end of the Tube (to which the Linnen was ty'd) under the Surface of the Water in a Glaſs; and *found that the Water did preſently begin to riſe.* The *very firſt Aſcent* was pretty conſiderable; For in the Space of 16 Minutes it had got up near one Inch and three Fourths. But as it *advanc'd ſtill higher*, its progreſs was *ſlower*, and that in ſuch Proportions as here follow.

At the end of 24 Hours, it had riſen but to 16 Inches; the *Bladder at the top being then near half fill'd with the Air*, which had quitted the Aſhes as the Water paſs'd thro' them. But here happen'd an Accident, which *prevented any farther Obſervation of the Swelling or Diſtention of the Bladder by this expell'd Air*. For the *upper part* of the Tube (to which the Bladder was ty'd) being crack'd round, ſoon after drop'd off. However, this hinder'd not *the Continuation of the Experiment with reſpect to the Aſcent of the Water*. For at 24 Hours from the *laſt Obſervation*, I found it had *gain'd 6 Inches more* in Height. And 'twas very eaſie to trace it in all its Motions, by *the change of Colour the Water gave thoſe Parts of the Aſhes it paſs'd through*, which render'd them very diſtinguiſhible from *thoſe which were yet dry*.

When 24 Hours more were compleated, the Water had riſen four Inches and an Half, and ſomething better. And at the ſame diſtance of Time again, it had aſcended three Inches higher. The ſucceeding 24 Hours brought it two Inches higher ſtill; and now it was gotten within Half an Inch of the Top of the Tube. In the Space of ten Hours more, it finiſh'd that little remaining part, and reach'd the Extremity of the Tube compleatly.

Such was the Progreſs of the Water, and at this rate did it make its way thro' the *compact Body of the Aſhes* with which the Tube was fill'd.

Having finiſh'd this Obſervation, I then reſolv'd to know, *what quantity of Water the Aſhes had abſorb'd*; in order to which I proceeded thus. I weigh'd a Glaſs of Water very nicely, and pour'd part of it into the Glaſs; wherein the Tube had all along ſtood, *till it reach'd the Mark* at which the Water ſtood, when the Tube was firſt immers'd in it. Then (weighing the remainder) *I found the Quantity of that pour'd forth* (which was therefore equal to that *abſorb'd by the Aſhes)* to weigh 1792 Grains; which is pretty nearly that of the *bulk of ſeven Cubical Inches.* Now the Capacity of the Tube (its Diameter being three Fourths of an Inch, and its Height 32) was about 14 Cubical Inches: So that the quantity of the Water equal to about one half the Content of the Tube, was drank up by the Aſhes.

I ſhall now take notice of the Particulars occurring in this Experiment, which ſeem to me to be well worth Obſervation: And theſe I ſhall offer *here by themſelves*; intending to make ſome *general Remarks upon this whole Claſs of Experiments*, after I have related all the Experiments that belong thereto.

Firſt, Tho' the Aſhes were ramm'd ſo very cloſe together, yet the *Interſticies of them were capable of admitting a Quantity of Water equal to half the Content of the Tube.* For the Content of the Tube was but little more than 14 Cubical Inches, and the bulk of Water abſorb'd was as good as ſeven; as was but now obſerv'd.

Secondly, The *Progreſs of the Water* through the Aſhes was *very diſproportional to the Times:* Becauſe 'twas found, that in the equal Intervals of 24 Hours, it made its way according to the following *Series*; *viz.* 16, 6, 4 $\frac{3}{4}$, 3, 2, (and in the laſt 10 Hours) half an Inch.

Thirdly, The Force with which the Water made its Aſcent, was very conſiderable; *being ſuch as was ſufficient to overcome the Reſiſtance of the Air impriſon'd in the Interſtices of the Aſhes, and to drive it away before it, towards the upper Part of the Tube.*

Now 'tis plain the Reſiſtance of the contain'd Air, was not a very ſmall one, from hence, that it was *ſuperior to that Force by which the Thorax is contracted, and the Air thrown out of the Lungs, in a ſtrong Expiration.* Becauſe when I endeavour'd to force Air by *my Breath*, thro' the Tube *not above half fill'd* with Aſhes, I could not preſently ſatisfie my ſelf that I did do it: Whereas we ſee the

Water eaſily made it ſelf a Paſſage, when the Tube was not only quite full, but alſo the Aſhes were preſs'd together, as hard and cloſe as poſſible.

But to put it out of all doubt, that the aſcending Water did actually meet with and overcome ſuch a Reſiſtance as what I ſpeak of, *viz. That of Air lodg'd in the Interſtices of the Body it paſs'd thro'*; let it be obſerv'd in the next place,

Fourthly, That *it was viſible by the gradual Intumeſcence of the Bladder* at the Top of the Tube, that the Air was really protruded out of the Aſhes by the Water as it aſcended along.

I believe none will Attribute this *Swelling of the Bladder* to any other Cauſe than the Force of ſome included Air, which ſtretch'd it, and plainly endeavoured to get away by ſo doing. And that it was forced out of the Aſhes by the Water, is as obvious as any thing can well be; ſince there could be no other poſſible Cauſe that ſhould expel it at that time. And beſides, in that it gathered more and more in the Bladder, as the Water advanced higher and higher, by that means it plainly *pointed out the Cauſe* which forced it thither.

Fifthly, The Water roſe not only in the *Aſhes adjoyning to the inward Surface of the Tube, but alſo thro' the whole Body of it*, and that *equally* too, (as appear'd upon examination.

Whatever therefore were the Cauſe of the Water's Aſcent, *that Cauſe acted uniformly*, ſince the Water was in all parts and places *equally influenc'd* by it.

Sixthly, The bulk of Air forc'd out of the Interſtices of the Aſhes, by the Water, we may conclude (and I think rightly) to be equal to that of the Water which ſupplied its place. And if ſo, then 'twas as much as half the Content of the Tube, or pretty nearly as much as the Bulk of Aſhes therein contain'd, (as follows plainly enough from the *firſt Obſervation.*)

Seventhly, The *Aſcent of the Water was by far ſwifter, when there was a much greater quantity of impriſon'd Air to oppoſe its Paſſage, by reaſon of the longer Column of Aſhes* (in which that was contain'd) than when it had made more way, and (by *getting higher in the Tube, having ſhortned the Column of Aſhes*) had a *leſs quantity* of Air to reſiſt it in its Motion upwards.

Query 1. Does not this Phænomenon of the Aſcent of the Water through the Interſtices of the Aſhes, *amount to the very ſame Caſe with that of its riſing in ſmall Tubes, or between two Glaſs Planes?* Do not the Particles of this Matter, by their little Hollows and Intervals, form a Congeries of *minute ſlender Pipes,* or *Surfaces very nearly approach'd to each other*; ſo that the Liquid riſes in each Caſe by virtue of *one and the ſame Cauſe?*

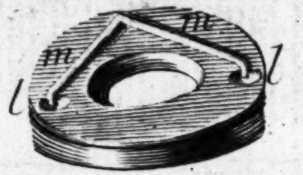
Query 2. Why is the Aſcent of the Water ſlower, the higher it riſes in the Tube?

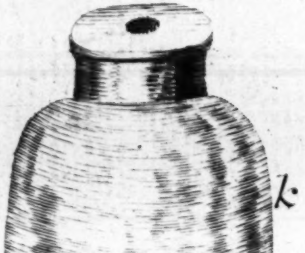
'Tis evident, that at firſt there is more intercepted Air to be remov'd out of the way, than afterwards, when the Water has ſhortned the Column of Aſhes.

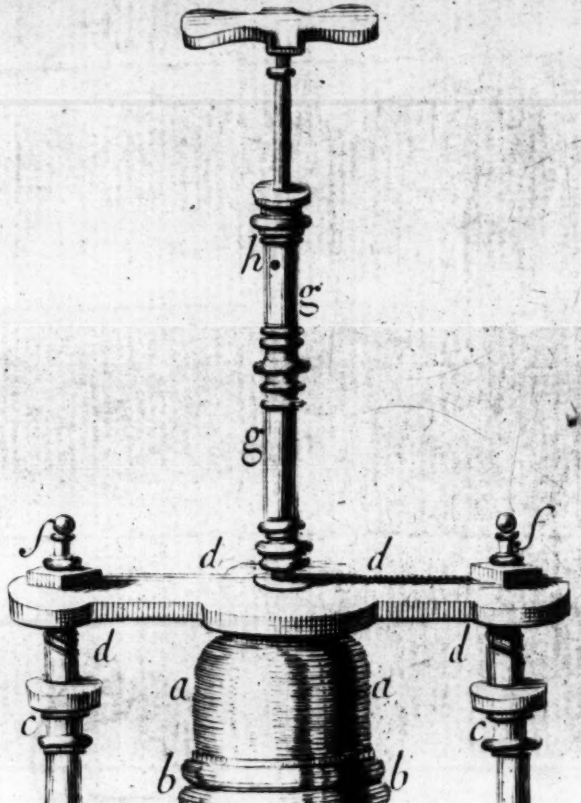
Is it therefore true, that the Water does at firſt actually meet with a more powerful Reſiſtance, and notwithſtanding riſes with more Velocity, than when 'tis leſs reſiſted? Or, ſhould we not rather conclude, that it does indeed meet with leſs Reſiſtance at firſt, than afterwards; and therefore, that *this intercepted Air is not in reality that Obſtacle, which at firſt ſight it appears to be?*

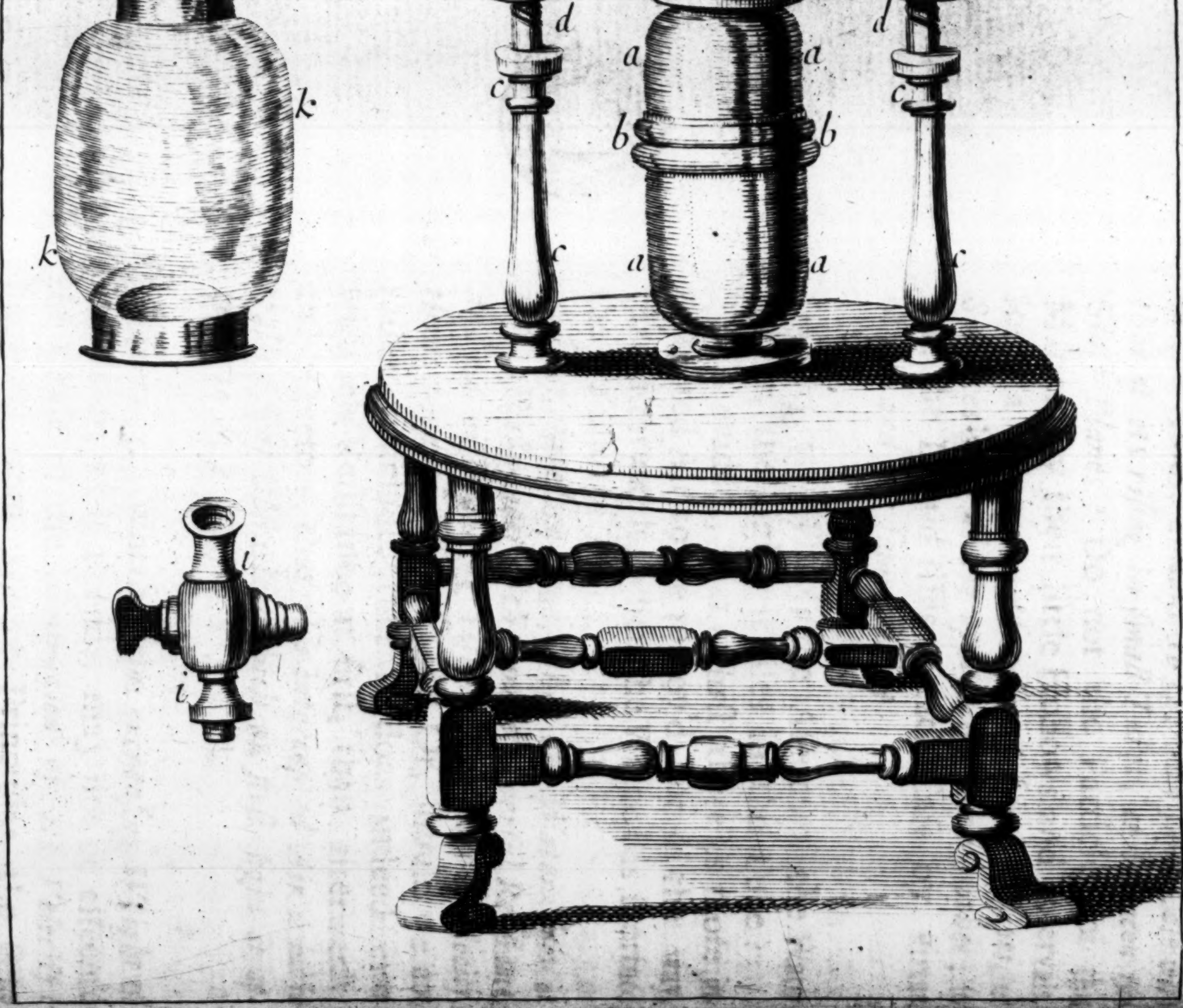
Query 3. Whether the *encreaſing Weight* of the Water, as it aſcends, may not be eſteem'd the Cauſe of the *diminution of its Velocity?* Becauſe, from *Statick* Principles, the ſame Power

TABU. VI.





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Power moving *different Weights*, ſhould produce different Rates of *Velocity*.

Experiment VI.

The Aſcent of Water *thro'* Aſhes *in* Vacuo.

HAving fill'd a Tube about 10 Inches in Length with Aſhes, (as before) it was plac'd in a *Receiver*, and the Air exhauſted. I ſuffer'd it to ſtand ſome time in that ſtate, to give liberty to the Air contain'd in the Aſhes to get away: Then plunging the lower End of the Tube under the Water, I found (according to my Expectation) that the Water roſe much faſter in that very much rarefied Medium, than in the open Air. Becauſe, in about four Hours time, it had mounted as high as it could go, having compleatly reach'd the Top of the Tube.

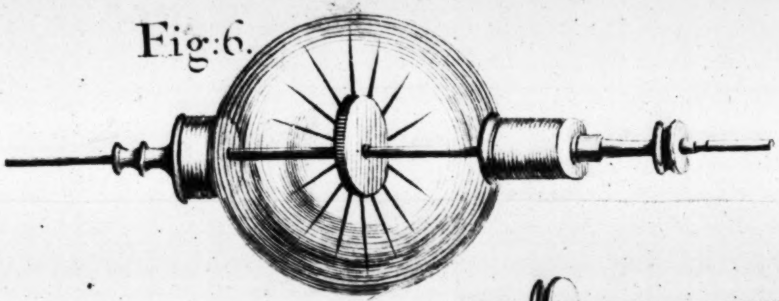
So that comparing the Reſult of this Trial with the former, we find that *Here* was a Height of 10 Inches ſurmounted in 4 Hours; whereas *There* 32 Inches took up 130 Hours to finiſh it.

By which Account it appears, that the *Heights* are in the Proportion of 3⅕ to 1, but the *Times* as 32⅓ to 1. So that the Water was more than 32 times as long in going (in *Common Air)* a Space *triple* to that which was finiſh'd in *Vacuo.*

But this Eſtimate of the ſwiftneſs of the Water's Aſcent would have been more exact, had it been obſerv'd, at what time preciſely the Water reach'd the ſame Height in both Tubes. *Ex. gr.* As here *in Vacuo*, the Tube made uſe of was 10 Inches long; ſo if it had been obſerv'd, in the other Caſe, at what time the Water had reach'd 10 Inches in that Tube alſo, (as it was obſerv'd at what time it roſe 16 Inches, *viz.* at the end of 24 Hours) then the Proportions of theſe different Times in which the Water had aſcended to the ſame Heights in both Tubes, would have given a nearer account of the *Velocities.* For if the Motions were equable, the Velocities would be juſt *reciprocally*, as thoſe *Times.* But if they were not uniform, yet the rate of the Swiftneſſes may be more nearly gueſs'd at, by taking the Liquid at the ſame Height in each Tube, than at different Heights.

TAB. VII.

Fig:6.



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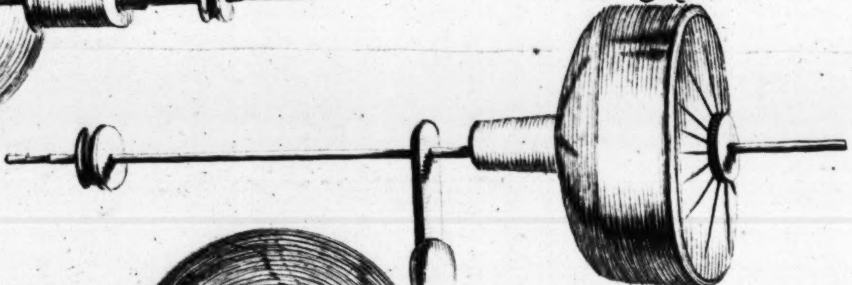

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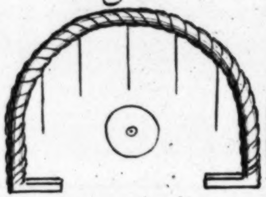

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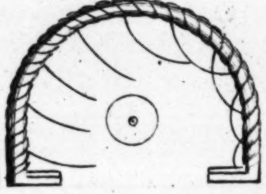

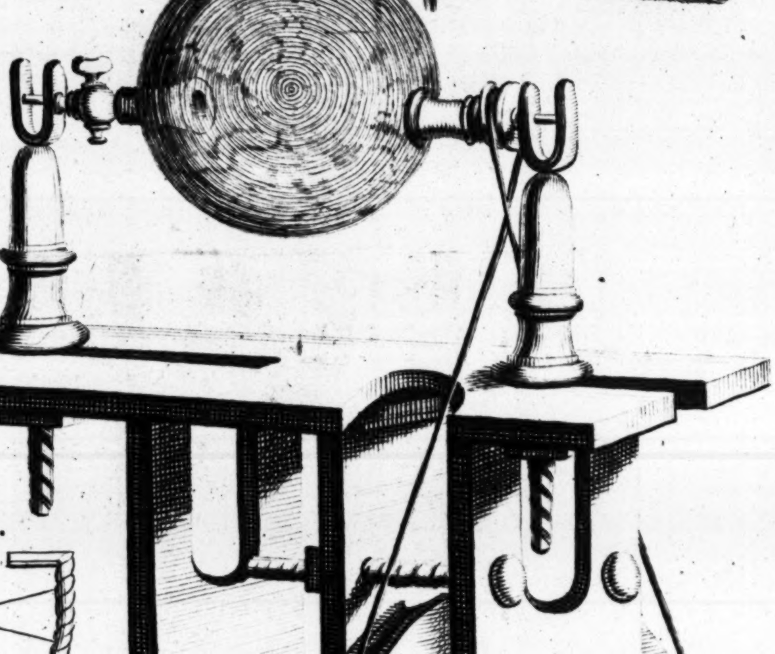

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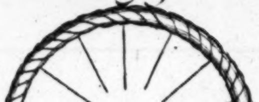

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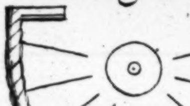

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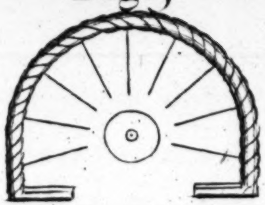

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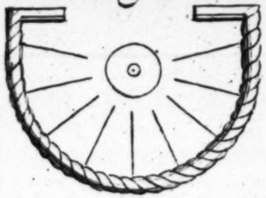

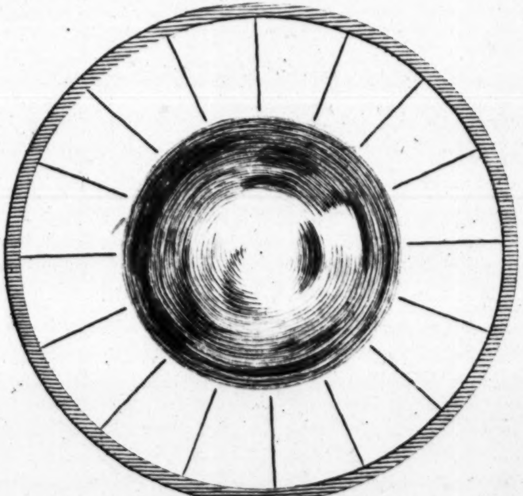

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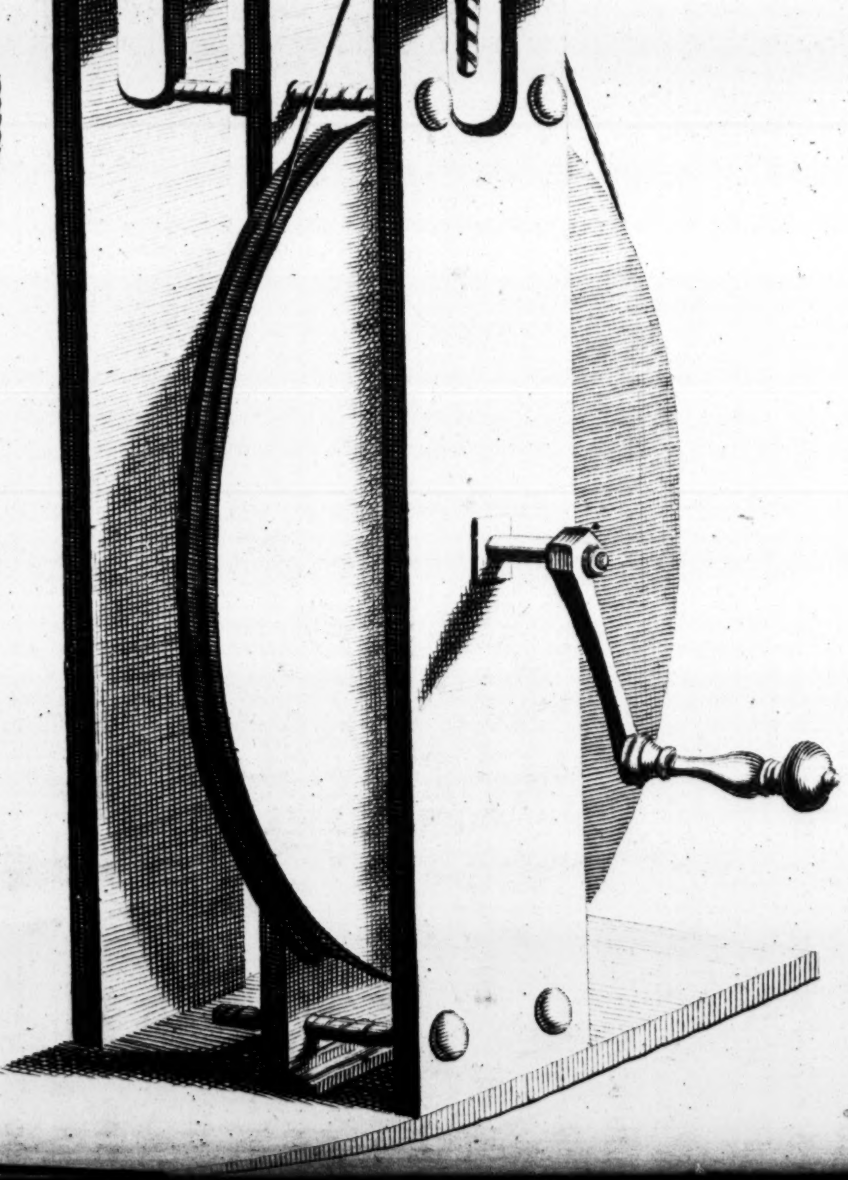

Experiment VII.

The Aſcent *of Liquors in ſmall Tubes, of* unequal Thickneſs, *but* equal Bores *or* Cavities.

HAving procur'd Two Tubes, the Diameters of whoſe Cavities were as nearly equal as they could be made, but one at leaſt ten times as thick as the other; I put them into the forementioned *Tinged Liquor.*

The Reſult was, That *there was no difference to be perceived between the Heights the Liquor had aſcended to in each Tube.*

Experiment VIII.

The Aſcent *of* various Liquors *between* two Square Glaſs Planes.

I Try'd this in *Spirit of Wine*, *Oil of Turpentine* and *Common Oil*.

All theſe roſe between the Planes, as the Tinged Water did. The Difference lay in this, that they all aſcended in an Entire Body from one ſide of the Planes to the other, without thoſe Interruptions and Intervals, which generally happen when the Water aſcends. And this, even tho' the Planes were held together, without any thing to ſeparate them and not only ſo, but alſo Forcibly preſs'd together: In which caſe they muſt needs touch in many Parts. And notwithſtanding that, the Courſe of the Liquor ſeem'd to be perfectly uninterrupted.

There was a remarkable Difference between the *Times ſpent* by the *Spirit* of *Wine*, and the *Oils*, in their Aſcents.

The *Common Oil* mov'd extreamly *ſluggiſhly* in compariſon with the *Oil* of *Turpentine* and *Spirit* of *Wine*; inſomuch that the Former was near an Hour in riſing as high as th

TAB. VIII.

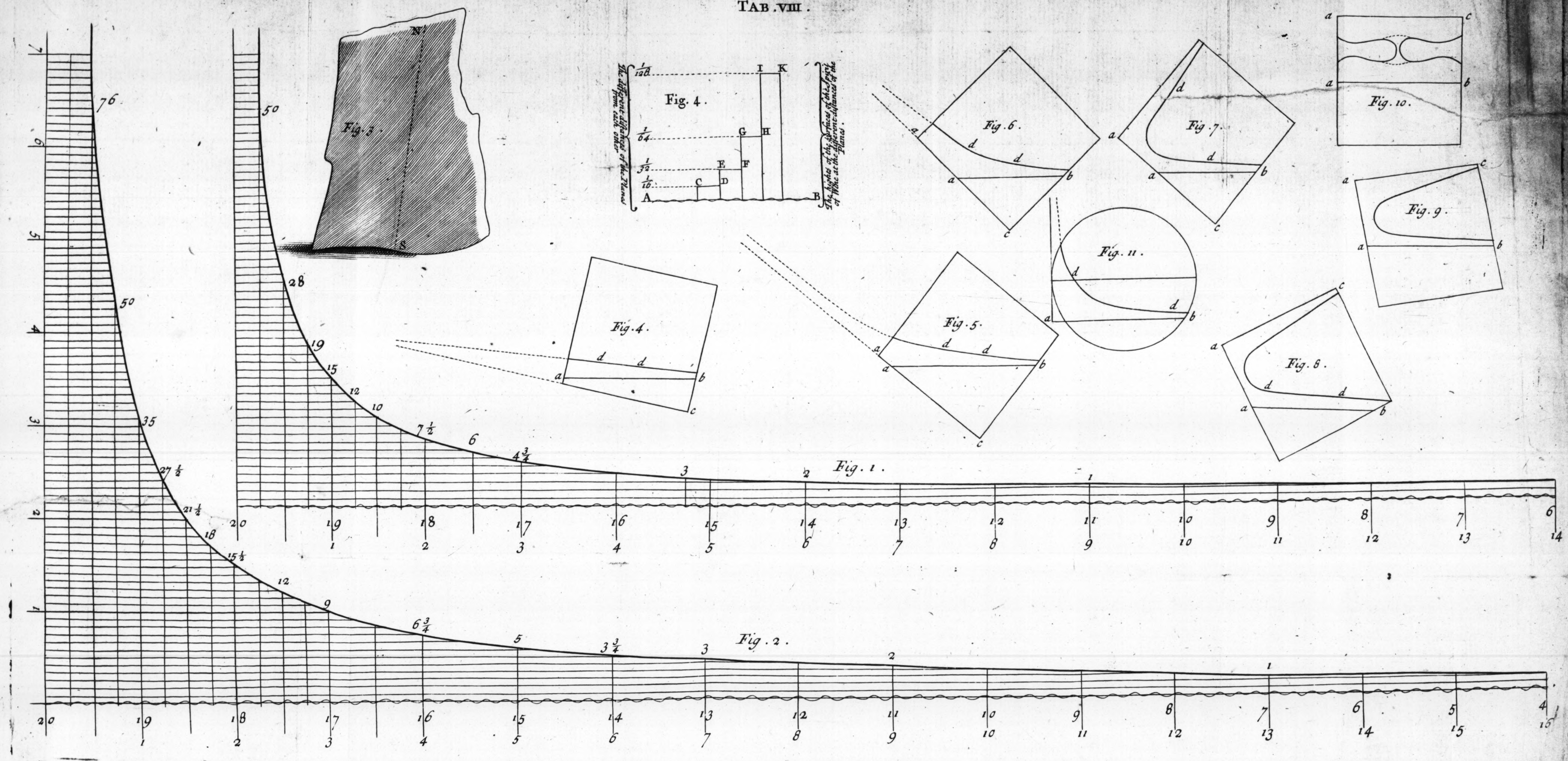
Fig. 1.
Fig. 2.
Fig. 3.
N
S
Fig. 4.
1/128
1/64
1/32
1/16
A
B
C
D
E
F
G
H
I
K
The different distances of the Planes from each other
The hights of the surfaces of the Spirit of Wine at the different distances of the Planes.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.
a
b
c
d
76
50
35
27 1/2
21 1/2
18
15 1/2
12
9
6 3/4
5
3 3/4
3
2
1
50
28
19
15
12
10
7 1/2
6
4 3/4
3
2
1
20
19
18
17
16
15
14
13
12
11
10
9
8
7
6
5
4
3
2
1
14
15

the Two Latter would do in leſs than half a Minute.

Having now given an Account of the Experiments themſelves, and ſubjoyn'd what *Remarks* had a *more immediate relation* to any of them *in particular*; I ſhall now make ſome general *Obſervations* upon the whole, and then conſider *how the Phænomenon it ſelf may be ſolv'd.*

Firſt of all then, we find, that this *Phænomenon* of the *Aſcent of Liquors* (between the Surfaces of nearly contiguous Bodies) *like that in ſmall Tubes*, *does no way depend upon any Action or Influence of the Air.*

For in all theſe Trials, the Liquor roſe with as much eaſe and freedom in an *exhauſted Receiver*, as in the *open Air:* But in *one* Caſe particularly, it aſcended with a vaſtly greater Velocity in ſo thin a Medium as that we call a *Vacuum*, than under all the Preſſure and vigorous Action of *common Air.*

There was indeed, ſome difference, with reſpect to the *Branching* and *Spreading* of the Liquor in its Aſcent; but this is a trivial Conſideration in compariſon with what ought chiefly to be regarded in this matter, and that is the *Height* and *Force* of the Liquor; which, *without* the Air, will be at leaſt (to ſay no more than that) as conſiderable as *with it.*

Secondly, *Some Liquids riſe after a manner very different from what others do.*

This is plain upon theſe two accounts :

1. Some Liquids, as they riſe, *branch* themſelves into *various little Streams or Rivulets*, and by that means leave (to all appearance) vacant Spaces and Intervals betwixt them: After which manner 'twas obſerv'd before, that the *tinged Water* roſe between the Glaſs Planes. But others again, mount up all in an entire Body, from one ſide of the Planes to the other; as *common Oil*, that of *Turpentine*, and Spirit of Wine.

2. Some aſcend with a *prodigious Swiftneſs*, in compariſon of others.

Thus, the Two laſt mention'd Liquors made at leaſt an hundred and twenty times as much haſte to get up between the Planes, as the former did (as is plain from what was before related of them.)

And perhaps *other Liquors* may be diſcover'd, which may as much exceed *theſe* in the Velocity of their Aſcent, as *they* did the *common Oil.*

And, it may be, *all imaginable Proportions of Velocity*, may be anſwer'd by thoſe of Liquors (of ſome ſort or other) *aſcending thus between the contiguous Surfaces of Bodies, or in ſmall Tubes.* For the Cauſe of this Phænomenon (if it be what I take it to be) *is capable of producing an infinite diverſity of Effects*, according to the *Difference of the Matter* it has to work upon.

Thirdly, *Liquids aſcend not only in* perpendicular *Directions, but in all imaginable Angles of Obliquity to the Horizon.*

For when the Experiment was made with the *round* Planes, the *tinged Liquor* immediately diffuſed it ſelf to the Extremities or Edges of them, every where through the whole Circumference.

Now the Liquid could riſe perpendicularly but in one Direction only, *viz.* that which we may conceive to paſs through the Center of the Two contiguous Circular Planes. In all the other Directions it muſt aſcend *obliquely*, diverging juſt as as an infinite Number of *Chords* in a *Circle*, drawn from the End of the *ſame Diameter.*

And ſuppoſing it reached *all* Parts of the Circumference at the ſame time (as it did, without the leaſt difference to ſenſe) we have then here, as it were, the Reverſe of *Galileo's* famous Propoſition, about the *Equitemporane-*

ous Deſcents of heavy Bodies in the *Chards of a Circle*: For in this Caſe, the *aſcending Liquid* deſcribes them all in equal times, as in that Caſe, the *deſcending Solid* does. And if the *one aſcends* and the *other deſcends*, by virtue of one and the ſame Cauſe (as I cannot forbear thinking but they do) then 'tis no wonder that there ſhould be ſuch an Agreement betwixt them, and that the *ſame Cauſe* ſhould produce a *ſimilar Effect*, both in *Solids* and *Liquids*, when *ſimilar Circumſtances* are ſuppos'd on both ſides. And it all amounts to no more, than *Attraction* upwards in one caſe, and downwards in the other; and this in the ſame ſort of Figure too, namely, a *Circle*.

Fourthly, This Phænomenon is not *confin'd to any one particular ſort of Matter*.

The Liquids roſe, not only between the *Glaſs*, but the *Marble* and the *Braſs* Planes too. And there's no doubt, but had the Experiment been made with Planes of various other ſorts of Matter, it might have ſucceeded in like manner. 'Tis poſſible, that *ſome Liquids* may not riſe between the Surfaces of *ſome Bodies*, which others will riſe freely between: Nay, I know not, whether inſtead of riſing, they may not be ſunk and depreſs'd. One and the ſame Cauſe acting in different Circumſtances, is capable of producing a great variety of Effects.

Fifthly, A *greater quantity of Matter* contributes nothing to the *riſing of the Liquid.*

This is plain from the Experiment of the two Tubes of *equal Cavities*, but *unequal Thickneſſes.* And by a *parity of Reaſon*, the Thickneſs or Thinneſs of the *Planes* ſhould produce no alteration, with reſpect to the Liquors aſcent between them.

Sixthly, The *the Aſcent of the Liquor is favour'd* and *promoted* by ſmall Particles of Matter laid in its way.

Thus the Water *aſcended higheſt*, when the Planes were rubb'd over with *Wood Aſhes.* Perhaps *other Matter* might be as great an *Impediment*; or *give more aſſiſtance to ſome Liquors in their Aſcent, than to others.* But theſe and many other things, (*which now I can but hint*) may perhaps, ſome time or other, be propos'd as Subjects of farther enquiry.

Having made theſe Obſervations (*General* and *Particular*) upon the ſeveral EXPERIMENTS propos'd, the next thing is the *Solution of the Phænomenon it ſelf.* And here I make no ſcruple, to reduce all the Varieties mention'd to the ſimple Caſe of *ſmall Tubes*; becauſe they all of them (as is plain by conſidering the Circumſtances) amount to no more than *That*.

For example: The two *Glaſs Planes* in theſe Experiments, being plac'd *very near* one another, compoſe a Tube *of the Form of a Parallelipiped*, whoſe thickneſs is exceedingly ſmall. So that therefore, having found *a Solution for the Phænomena of ſmall Tubes*, the ſame may eaſily be accommodated to all the reſt.

To proceed then; It appears evident to me that *the Principle we ought to have recourſe to in this Caſe, is no other than that of Attraction.*

A Principle which governs far and wide in Nature, and by which moſt of its Phænomena are explicable. I know very well there have been Attempts made, to ſolve *this Appearance* diverſe other ways. Some have argued from the *impeded* or *diminiſh'd Action of the Air*; others from the *Innixion or Reſting of the Parts of the Fluid, on the Pores and Aſperities of the Glaſs*; others again from the *Congruity and Incongruity of the Parts of Matter* one to another. This *laſt* Notion, without farther explication, is ſomewhat more unintelligible than the two former: And tho' perhaps they are all of them wrong, yet the *Firſt Two* ways of ſolving the Difficulty have this Advantage above the other, that they are *perſpicuouſly* Falſe; whereas this latter is more *myſteriouſly* ſo, leaving the Underſtanding in ſome doubt, whether it may be True, or no; becauſe of the hard Words of *Congruity* and *Incongruity*, which being not explain'd, *may* *poſſibly* carry ſome better meaning along with 'em than they ſeem to promiſe. If it ſhould be thought that *Attraction* is a Word no leſs hard and unintelligible than the former are, I can only ſay this, *That 'tis plain Fact that there is a Power in Nature, by which the Parts of Matter do tend to each other* ; and that not only in the *larger Portions or Syſtems of Matter*, but alſo the *more minute* and *inſenſible Corpuſcles*. And that the Law which obtains in the former Caſe (*viz.* amongſt the greater Bodies of the Univerſe) is fully determin'd and ſettled, namely, that the Attraction or Centripetency decreaſes reciprocally, as the Squares of the Diſtances (of the Attracted from the Attracting Bodies) do encreaſe. But the Law by which the ſmaller Portions of Matter tend, to each other, is not ſo compleatly ſettled but left yet for further diſcovery ; only 'tis known, that it muſt be very different from the other, and *that the attractive Forces here do decreaſe in a greater proportion than that by which the Squares of the Diſtances do encreaſe:* but the nature of *that Proportion*, or how *complicate* it is, or what *Varieties* there may be in it, is not yet accounted for; nor will not eaſily, becauſe of the ſeeming *invincible Difficulties that attend the making Experiments* and Obſervations requiſite to ſettle *ſo nice* a Point. Only the Fact it ſelf is paſt diſpute, and the Diſcoveries made by *that very great Man*, Sir ISAAC NEWTON, (*the Ho-*

nour of our Nation and Royal Society) have ſet *both theſe Laws of Attraction* thus far, in a very clear Light to all that will uſe their Eyes to ſee them.

Now ſince we are certain there is ſuch a Principle in Nature, and one ſo *Extenſive* and *Predominant* too, as that of Attraction; I think it would be a ſatisfactory Proof enough of the Intereſt of that Principle in this Phænomenon, to ſhew that it may be handſomly accounted for by it, without being forc'd upon any of thoſe *obſcure precarious Suppoſitions*, which in other Solutions a Man can't well avoid.

However, before I do this, I ſhall argue the Point another way, and by ſhewing ſome remarkable Agreements of this Phænomenon, with others in which *Attraction* is moſt evidently concern'd, do ſomething, I hope, to perſwade a belief of the Intereſt of the ſame Cauſe here alſo.

What I propoſe to conſider at preſent is the *Magnet* or *Loadſtone*, ſome of whoſe Effects coincide with thoſe of *ſmall Tubes* to a Wonder.

1. A Loadſtone of any Form whatſoever, will attract *Iron*.

So we find that Bodies ſet together after any manner, or in any figure whatſoever ſo they do but compoſe a *ſmall Tube*, or what is equivalent thereto, will give occaſion to the Liquor to aſcend between their Surfaces.

2. The Magnet exerts its force as well in *Vacuo*, as the *open Air*.

And we find that Liquids riſe as freely in the one as the other of theſe; in the moſt thin and rarefied Medium, as well as the moſt groſs and denſe.

3. *Small Loadſtones*, for the generality, have a *ſtronger* attractive Power (in proportion to their bulk) than the large ones have: And ſo *ſmall Tubes* will make the Liquid aſcend higher than great ones will. And as the inward Cavities and Surfaces are leſſen'd, ſo the Liquid will riſe *higher* and *higher*.

4. If a Loadſtone be divided into ſeveral parts, or ſmall Loadſtones, theſe little ones (ſuppoſing the virtue of the Stone to be *equably* ſpread thro' the Body of it) will all together ſuſtain a vaſtly greater weight of Iron, than the one great one alone before would do; tho' taken *collectively*, they contain very nearly the ſame quantity of Matter with it.

So, were a Tube of a *very ſmall Bore*, but a *great Thickneſs*, to be divided into ſeveral Tubes or *parallel Surfaces*, the Quantities of Water ſuſtain'd in all of them together, would vaſtly tranſcend *that*, which was ſuſtain'd when they were all *united together*, and compos'd but one ſingle Tube.

So that *in ſmall Tubes, as well as Loadſtones*, the encreaſe of Superficies is That on which the Encreaſe of the Force ſeems mainly to depend. Nor *does this Aſſertion any way thwart* what was *ſaid before*, *viz*. That as the *Surfaces did decreaſe*, ſo the *Aſcent of the Liquid* would be more and more conſiderable: For *there* I only compar'd the Effects of Tubes of *different Diameters* one with another; and ſhew, that the leſſer Tube has the Advantage of the greater: But *here* I compare the Effects of *innumerable little Tubes, all made out of one ſingle Tube*, with the Effect of that ſingle Tube it ſelf; and ſhew, that the *Encreaſe of Superficies* conſequent upon ſuch a *diviſion*, gives the *Aggregate*, or Collection of Tubes, (by many degrees) the Advantage of a *ſingle* one.

Now, upon theſe Accounts, I think, I have ſome Grounds to believe, that the Phænomena of the *Load-ſtone*, and of *ſmall Tubes*, depend upon one and the ſame Principle in general: For here's a ſurprizing *Correſpondence of Effects*; why then may they not agree in their *Cauſe*?

But to proceed. That *Attraction* I ſpeak of (as the Cauſe of the Aſcent of Liquids in ſmall Tubes) I make to proceed (mainly, if not ſolely) from the *Innermoſt* or *Concave* Surface of ſuch a Tube; and not from the *Solidity* or *Quantity* of Matter which it contains. And a Proof that this is ſo in Fact, may be deduc'd from the *Experiment of the Two Tubes* before-mentioned. But more than this, that it likewiſe *ought to be ſo*, appears from hence, That *the Attractive Power of ſmall Particles of Matter acts only on ſuch Corpuſcles as are in contact with them, or remov'd but at infinitely little Diſtances from them.*

Upon which account, I think, I may ſay, that the Remoter Surfaces of the Tube, between the *innermoſt* or *Concave* one, and the *outermoſt* or *Convex* one, do contribute nothing to the Effect; that is, the Liquid is not influenc'd by any Attraction of Theirs.

Theſe things premis'd, let us in the next place conſider how this Phænomenon may be accounted for by *Attraction:* That is, How the Liquid may riſe in a ſmall Tube, by the Attraction of the Parts thereof by the Concave Surface of the Glaſs.

Let ABCD be a *ſmall Tube*, perpendicularly immers'd in a Liquid, whoſe *horizontal Surface* is ECDF.

The Parts of the Liquid at *a a*, *b b*, adjoining to the concave Surface of the Tube, are *ſtrongly attracted* by it, and that in a *Direction perpendicular* to the Sides of the *Cylindrick* Glaſs; or (which is all one) *parallel* to E F, the Surface of the Liquid.

Now the Particles *a a*, *b b*, gravitating in Directions perpendicular to E F, that is, parallel to A C and B D, the Sides of the Tube; by means of the aforeſaid Attraction, it comes to paſs, that the Particles *a a*, *b b*, have all of them a much leſs *Momentum* or *gravitating Force*, than otherwiſe they would have, were the Attraction away.

Therefore the Parts of the Fluid, which lie immediately *under them*, are much leſs preſs'd upon, than otherwiſe they would be.

And altho' the Particles *d d* lie farther out towards the Middle of the Tube; yet in a very minute and ſlender one (ſuch as we here ſpeak of) they are near enough to be within the reach of the powerful Attraction of the Surface, ſo far as to be in ſome meaſure influenc'd thereby; either *immediately* or *mediately*, by the means of the Particles *a a*, *b b*, which are ſtrongly urged towards the Glaſs, and do (by the General Law) attract the neighbouring Particles *d d*, towards themſelves.

Upon theſe accounts, the *Momenta* of all thoſe Particles, comprehended within the Circumference of the lower Baſis of the Tube, being much leſſened; the Fluid, which lies directly

Fig: 1.

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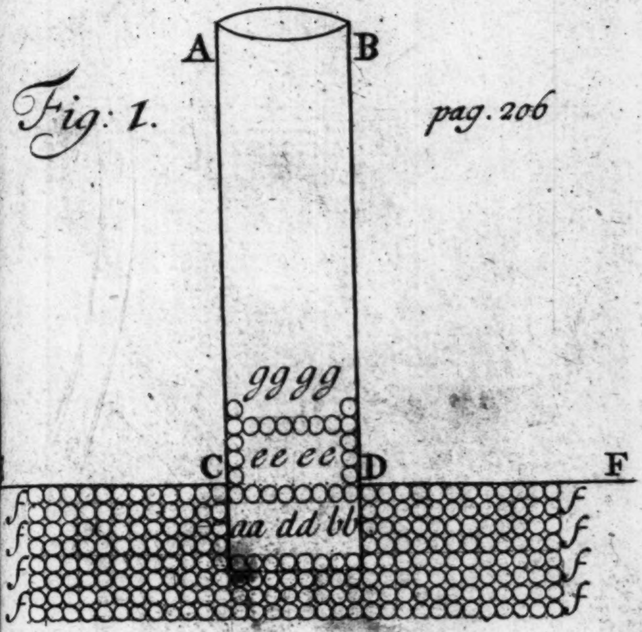

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directly under them, is proportionably leſs preſs'd.

But the Parts of the Fluid *ffff*, which lie between the Surface ECDF and the Bottom of the Tube, at more remote diſtances from the Sides of the Tube than its Semidiameter: Theſe Particles, I ſay, being out of the reach of any ſuch Attraction, do *gravitate* with their whole Force or *Momentum* on the Parts which lie under them.

Therefore it appears, that by the *Immerſion* of the ſmall Tube into the Liquid, the *Equilibrium* is deſtroy'd between thoſe Parts of the Liquid lying *within* the Circumference of the lower Baſis, and thoſe which are placed *without*.

Therefore, by the *Hydroſtatical* Laws, the Liquid muſt riſe within the Surface of the Tube: For the *ſtronger Fluid* will ſtill preſs in upon the *weaker*, and force it away before it. That is, the Particles about *a a*, *b b*, principally; and next to them, the Particles about *dd* muſt neceſſarily give way to the Particles below them, which are urged on by the Superior *Momentum* of the Particles which come from the aforeſaid remote Diſtances about *ffff*.

From whence it follows, that thoſe Particles about *a a*, *b b*, muſt neceſſarily aſcend higher in the Tube, as to *e e e e*.

When they are riſen higher, the Attraction to the Sides of the Tube will take place as before; and by leſſening their *Momenta*, with reſpect to thoſe *below* them, will give a new Occaſion to the *External* Fluid, to inſinuate it ſelf within the Bottom of the little Tube, and conſequently to puſh thoſe Particles ſtill up higher, as to *gggg*.

Thus by the continu'd Action of the ſame *Cauſe*, the ſame *Effect* follows, and the Liquor continues to aſcend in the Tube, till it comes to a certain determinate Height, where it keeps its Station, and that by virtue of the *ſame Laws which brought it thither*.

And this, I hope, may be a ſatisfactory Account of the reaſon of this Phænomenon; and ſerve to ſhew the Manner and Way by which Liquids may be conceiv'd to riſe in ſmall Tubes. I muſt ſay, it appears to me to be the True one.

N. B. I ſpake but now (with a particular Remark) of the Preſſure of thoſe Particles of the Fluid, which lie at more remote Diſtances from the Tube, than its Semi-diameter.

'Tis to the Energy and Force of theſe, that the Aſcent of the Fluid ſeems *chiefly*, if not *entirely*, to be owing. For thoſe Particles nearly adjoining to the Convex Surface, are attracted in ſome degree, as well as thoſe which are approach'd to the *Concave:* And therefore can't be imagin'd to have any ſuch preponderating *Momentum*, as to force thoſe within to aſcend in the Tube.

But ſuch Particles as are placed at farther Diſtances beyond the Convex Surface, ſuffer no Attraction from it and ſo are ſufficient to preſs the Liquid away before them.

But it may be ſaid here, that if the ſurrounding Parts of the Fluid *without*, on the *Convex* ſide, were attracted as well as thoſe *within*, on the *Concave*; then the Fluid ought, by theſe Principles, to riſe *without*, on the Convex Surface of the Tube, as well as it does *within*, on the *Concave*: Which Experiment ſhews that it does not.

But (in anſwer to this) the Reaſon why it ought not to do ſo (or at leaſt very inconſiderably) is obvious, I think, from the different Natures of Convexity and Concavity.

Suppoſe we a ſmall Particle of a Fluid, in *contact* with the *Convex* Surface of a Tube: 'Tis plain, that all the *Lineolæ* or *ſtreight Filaments* compoſing this Surface, are *averted*, or turn'd from the aforeſaid Particle, except that one ſingle Filament, in which it touches the Surface.

But on the *Concave* ſide, all the Filaments are turned towards ſuch a Particle, which we imagine to be in *contact* with the Surface there.

From whence ariſes an exceeding great difference between the Attractions of a Particle of Fluid Matter, by the Filaments on the *Convex*, and on the *Concave* ſide.

For in the *Former* Caſe, the ſaid Particle muſt be vaſtly more out of the reach of theſe Attractions, upon the account of the averted Poſition of the Filaments, than in the *Latter* Caſe, where the Incurvation turns the attracting *Lineolæ* towards it, and by that means preſenting a *much greater Force*, produces a proportionally greater Effect.

And for this reaſon, the Parts of the Fluid *within*, loſing beyond Compariſon more of their *Momentum* than thoſe *without* do; the Fluid ought to riſe beyond Compariſon more on the *Concave*, than on the *Convex* Surface of the Tube. That is, its aſcent on the *Outſide* ought to be ſcarce *ſenſible*; and I believe it will be always found to be ſo.

Thus far we have ſhewn the Reaſon why the Liquor muſt riſe in the *ſmall* Tubes.

From hence now it follows likewiſe, that *it muſt of neceſſity riſe higher in Tubes of a ſmaller, than thoſe of a larger Bore.*

For ſuppoſe there be *two Forces*, each of which is to lift a *ſeveral Weight*. Now, from the Principles of *Mechanicks*, *that Force which bears the greateſt Proportion to its Weight, will be able to raiſe that Weight higher, than that Force which bears a leſs Proportion to its Weight, will*

will be able to raiſe that Weight. Let us alſo take two Tubes of *different diameters*, and the *ſame height:* The *attractive Powers are as the* Surfaces; and the *Weights* or Quantities of Liquor to be raiſ'd into theſe *two Cylinders*, are as their *ſolid Contents.* Or, becauſe the Heights being the ſame, the *Surfaces* are as the *Peripheries*; and the *Contents*, as the *Area's* of the *Baſes*;) the *Attractive Forces* will be as the *Peripheries*; and the *Weights*, as the *Area's* of the Baſes.

But there is a greater *Proportion* between the *Periphery* and the *Area* of the Baſe in the *ſmall* Cylinder, than there is between the like Quantities in the *great* one. Therefore in the *ſmall* Tube, the Attractive *Force* bears a *greater Proportion* to the *Weight of the Liquid to be raiſ'd*, than it does in the *great* one. And therefore the *Liquid muſt riſe higher in the former*, than in the *latter*.

From hence likewiſe we may form *a Rule, that may help towards determining the Height the Liquid muſt aſcend to, in any given ſmall Tube.*

For the Liquid muſt neceſſarily riſe till it comes to ſuch a Height; *that the Momentum of all the Liquid in the Tube, as it is there diminiſh'd by the Attraction of the Surface*, becomes equal to the *undiminiſh'd Momentum* of the External Liquid, at that depth the Tube is immers'd to. And when it is come to that particular Height, it muſt as neceſſarily ſtand, and go no farther.

And in that Caſe, the Proportion will run thus ---. As the *Diminiſh'd Gravity* of the Liquid in the Tube, is to the Abſolute Gravity of the *Collateral Cylinder* of External Liquid; ſo is the Depth of Immerſion, to the Height of the Liquid in the ſmall Tube. For, I ſuppoſe the Cylinder of the Fluid in the Tube to be balanc'd by one without, which has the ſame Baſe and whoſe Height is equal to the Immerſion; for the Baſes being the ſame, the Heights are as the *Contents* or *Quantities of Matter*. And to make an *Equilibrium*, or *Equality of Momenta*, the Forces muſt be *reciprocally*, as the *Bulk* or *Quantities*; that is (in this caſe) reciprocally as the *Heights*.

Now, as for the Reaſons inducing me to propoſe a Solution of the Phænomena of *Capillary* Tubes, *on ſuch Principles as I have here done*; I think it not improper here to ſubjoin them.

'Tis true, the *direct* and *ſeemingly-ſtreight* aſcent of the Liquid, from the *lower* to the *upper* parts of the Tube, would, at firſt view tempt one to think of no more than only *an Attraction upwards*, or in Directions *parallel* to the ſides of the Tube, inſtead of *perpendicular* thereto; and ſo to derive the Fluid's aſcent only from this, without any regard to *Hydroſtatical* Laws, or the *loſs and recovery of an Equilibrium*. But in this way there appear'd pear'd ſeveral Difficulties, of that ſtrength, that I could by no means get over them; and the conſideration of theſe, determin'd me to ſolve the Phænomenon the other way.

For, 1. I could not ſee any Reaſon to convince my ſelf, why a Particle of the Liquid, as *a* or *b*, which is there at that point in actual contact with the Glaſs, ſhould not be attracted to and by that Particle, rather than by another above it, and remote from it: Or at leaſt, why it ſhould not be vaſtly more attracted by that Particle with which it is in contact, than by another above it; and conſequently, why the Attraction ſhould not firſt begin, in lines *perpendicular*, and not *parallel*, to the ſides of the Tube, whatever be done afterwards: For I do not abſolutely exclude the *former* Attraction from being ſome way concern'd, tho' I make the *latter* the main and principal Cauſe of the Aſcent.

2. If a Body reſting on an *Horizontal Plane* were to be drawn (in a Direction parallel thereto) *againſt an upright or vertical Plane* erected upon the ſame Horizontal Plane; it appear'd certain to me, that the *actual Preſſure* of that Body in the *Horizontal* Plane would be diminiſh'd, according to the force wherewith the *Thread* was drawn, *which preſs'd it againſt the vertical Plane*. For it may be drawn ſo hard againſt the upright Plane, that

the Horizontal one ſhall have little or nothing at all of the burden of it. And applying this to the Caſe in hand; I concluded, that for this reaſon the Momentum of the Parts of the Fluid *aa*, *bb*, muſt be abated, with reſpect to the Particles lying immediately under them.

3. When I ſuppos'd the Liquor to aſcend purely by virtue of the Attraction *directly upwards*, in Lines *parallel to the ſides* of the Tube, I could not ſee a reaſon, *why the Liquid ſhould ever ſtop in any Capillary Tube* before it comes to the *very top of all*; which yet Experience ſhews it does. For if the Attraction be ſolely from the upper parts, then as long as there is any part of the Surface left unoccupied by the Liquid, *ſo long there is a Cauſe left in being, of the Liquids farther Aſcent.* And if there be that Cauſe in being, why ſhould it not exert it ſelf, and make the Liquid riſe, as well when 'tis gotten up 2 or 3 Inches high in the Tube, as when it was below at the bottom? All *Circumſtances here*, are the ſame as *there*, as far as I can with all my Attention diſcover. And that the Liquid *has already poſſeſs'd* ſome part of the Surface, can be no reaſon why *that part which is not poſſeſs'd*, ſhould not exert its Attractive Force, and draw up the Liquid, till it is gone as high as it can go; that is, till the whole Tube is full.

In a word, Why ſhould I deny the *upper part* of the Tube that Attractive Power, which I *ſo freely allow to the lower?* I know no reaſon, *to imagine this* Virtue *to lie ſo unequally ſcatter'd about the Tube*; and if it be not ſo, then, I think, I ought to expect the ſame Effect ſhould take place in the upper parts of it, that does in all other places.

And therefore, 4. By the Explication which I have here given of this Phænomenon, I found I could give a Reaſon *for the Aſcent of the Fluid to a determinate Height in any Capillary Tube propos'd*; which I ſaw no way, that was *tolerably Philoſophical*, to do by the *other Method*.

For the *Momentum of the external Liquid* being in ſome meaſure taken into this Account, as well as the *Attraction of the Tube*; the *Hydroſtatical* Laws, by which I ſuppoſe the Liquids *within* and *without* to be *balanc'd*, will themſelves *determine the Height to which the Fluid muſt riſe in the Tube*. Otherwiſe, as I hinted before, 'tis not a ſlight Difficulty, to conceive any other Limits to bound the Motion of the Liquor upwards, but the very top of the Tube it ſelf: And there indeed it muſt ſtand for a *good Reaſon*. But if it ſettles at any determinate depth *below that*, and this by virtue of the *direct* or *upright* Attractions, I muſt needs ſay, 'tis a Riddle, out of the Intricacies of which I have little hope to deliver my ſelf.

For the remaining Part of the Concave Surface lying above the Liquor in the *Tube*; either has, or has not, the Power of *Attraction*, like the other Parts of the Surface *below*.

If *not*, By what Law is a *Tube* ſo divided into *attractive* and *non attractive* Segments? How is the Limit between theſe two very Heterogeneous Parts determin'd?

Was there Originally and *always* ſuch a Diſtinction? Or, did it commence when the Glaſs was firſt blown into the Form of a *Capillary Tube*? Or, did it begin to take place only when the Tube was actually immers'd in the Liquid?

In ſhort, Does this ſtrange Property owe its Riſe to *Nature* or to the *Fire*; the *Blaſt* which made it a Tube, or to the *Water* 'tis plung'd in, when the Experiment is to be perform'd? If it be not to one of theſe, I am at a Loſs for its Original.

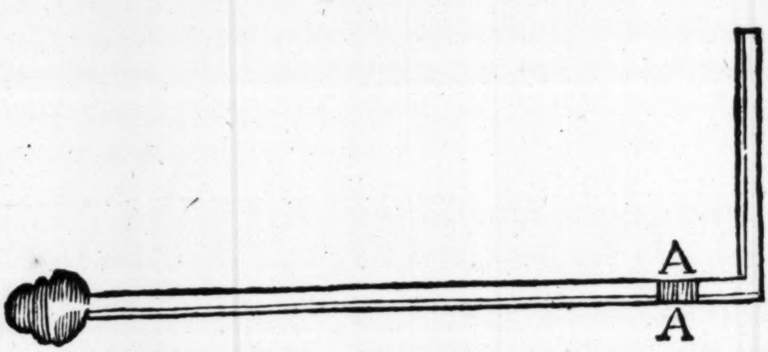
On the other hand, If the remaining part of the Tube, above the Surface of the Liquor where it ſettles, be endow'd with an attractive Force, *ſimilarly* and *proportionally* to the reſt, why does it not exert it ſelf, when the Liquor is fairly preſented within the Sphere of it; in like manner as the Attraction of the other Parts *below* did, *when the Fluid was brought within their Sphere*?

By how much the greater a Difficulty it will be, to anſwer theſe Conſiderations upon Juſt and Philoſophick Principles; by ſo much the more, I hope, will the former Solution I have given, appear to be clear and natural.

I could proceed to ſhew farther, how the other *Phænomena* of Small Tubes might very naturally be ſolv'd from *theſe* Principles; but it being very eaſie to make that Application, I ſhall wave all Diſcourſe of thoſe matters; and with that I have now ſaid, conclude this preſent Subject.

An Account of an Experiment concerning the different Denſities of the Air, *from the* greateſt *Degree of* Heat, *to* that *of* Cold, *in our* Climate.

I Took a Glaſs Tube about two Foot long, and of about one Fifth in Diameter; which I bent into the Form of a *Rectangular Syphon*, at nearly the Diſtance of ſix Inches from one End, thus:



At that Extremity of it which was fartheſt from the Angle, I cemented on a *Braſs Screw* with a ſmall Perforation in it; by which means, when I had put a little *Quickſilver* in at the ſhorter Leg, by inclining the *longer* Leg *this* or *that* way, I could bring the *Mercury* to reſt any where at pleaſure; as at A A.

The little Column of Mercury I here made uſe of, was about half an Inch in Length, as being ſufficient for the purpoſe of this Expeririment. Then having ſcrew'd a *Cap* on to the longer or *Horizontal* Leg of the *Syphon*, and the Mercury being fix'd and ſettl'd in a *certain* Part, I convey'd it into a *Trough*, together with a *Thermometer*, and pour'd on as much *warm* Water, as would cover the Ball of the *Thermometer*. Thus the *Syphon* lay, with its longer Leg under the Water, in an *exact Horizontal* Poſition, and its ſhorter Leg *upright* and *above* the Water. And the Reſult of this Contrivance was the Prevention of ſome Inconveniencies, which would have follow'd upon placing the Syphon ſo, as that the Water ſhould have found its way into it.

Beſides, the Action and Power of the *External Air* could not have been ſo certainly argu'd and determin'd, in *that* Caſe, as it might have been in *this*; where it had liberty to preſs as it would, and that *immediately*, without oppoſition or hindrance from an intervening Body. The *Spirit* in the *Thermometer*, being quickly influenc'd by the *warm* Water, I ſuffer'd it to riſe up as high as the little Ball on the Top of that Inſtrument, and indeed to paſs into it; that ſo I might make my Obſervations on its Deſcent with more Exactneſs. For I imagin'd, that by that time the Spirit was fallen to ſome convenient Degree, deſigned to begin the Account at, it might have acquir'd quir'd a pretty equal Degree of Heat in all its Parts.

Accordingly, I began my Obſervations, when it had deſcended to 130 Degrees above the Freezing Point; at which time I found the Length of the Column of Air, from the cloſed End of the Syphon to the neareſt Surface of the Quickſilver, to be juſt 144 Tenths of Inches. After the Spirit had deſcended 10 Degrees lower, the Air, which before poſſeſs'd 144 Parts, lack'd one of them now; and ſo on ſucceſſively at every 10 Degrees Deſcent of the Spirit, the Column of the contain'd Air was leſſen'd in its Length one exact Tenth.

When it had deſcended to 30 Degrees above the Freezing Point, the Air was found to poſſeſs but 134 of the forementioned Parts.

So that from hence it will be eaſie to conclude, that at the Freezing Point, the Air in the Syphon would be reduc'd to three Tenths leſs than at the laſt Obſervation; and conſequently at 50Degrees below the Freezing Point(which I am inform'd, is the greateſt Degree of Cold that has happen'd in our Climate) it would be reduc'd to 126 parts of the whole, and in that State would be one *Eighth* more denſe, than when at the greateſtDegree of our NaturalHeat. And the reaſon why I could not prove this latter part by Experiment, was, that when I came to expoſe the *Thermometer* and *Syphon* in the open Air, or Freezing Mixture, the Syphon would inſtantly receive the Impreſſion of the

Cold, and the Air contain'd in it be conſiderably contracted, before the *Thermometer* gave any ſign of ſuch Alteration. But ſeeing the former part of the Experiment ſucceeded ſo very regularly as it did, I think, there can be no doubt of the Truth of the whole Calculation; nor do I yet ſee how it could be better perform'd.

I ſhall add a Table of the different Degrees of the Air's Denſity at every 10 Degrees, from 130 above the Freezing Point, to 50 Degrees below it.

This Experiment was made *February* the 11*th* 1708; the Mercury in the Barometer (at the ſame time) ſtanding at 30 Inches.

	Degrees.	Parts.	
Above.	130	144	
	120	143	1/144
	110	142	1/72
	100	141	1/48
	90	140	1/38
	80	139	1/28,8
	70	138	1/24
	60	137	1/20,5
	50	136	1/18
	40	135	1/16
	30	134	1/14,4
	20	133	1/13.09
	10	132	1/12
Freezing Point.	0	131	1/11,08
Below.	10	130	1/10,3
	20	129	1/9,6
	30	128	1/9
	40	127	1/8,4
	50	126	1/8

This Tableſhews the Difference of the Air's Denſity at every 10 Degrees, from 130 above the Freezing Point, to 50 below it. As, ſuppoſing the Spirit in the Thermometer ſhould ſtand at 40 Degrees above the Freezing Point; I find right againſt it, in the third Column 1/16: Its State being then ſo much more Denſe than when the Spirit is elevated to 130 Degrees. And ſo of all the reſt.

The Second Column ſhews the Extent of the Air at the ſeveral Stations, from the *Greateſt* Heat, to the *Greateſt* Cold.

N. B. For the *better* underſtanding of the foregoing *Experiment*, I would note ſomething concerning the *Poſition* and *Motion* of the *little Column* of *Mercury* in the *Horizontal* Leg of the *Syphon.*

This Body of *Quickſilver* being fix'd in a certain part of the Tube (as, ſuppoſe *thereabouts* where it *now appears* to be in the *Fig.*) was *afterwards*, by the Rarefaction of the Air (contain'd between it, and the *End cover'd with the Cap*) driven farther towards *the Angle of the Syphon:* For the Mercury, *when put in*, neceſſarily *forcing the Air along* before it, there muſt needs be a Column of Air *included between it* and the *End of the Syphon* guarded with the Braſs Cap; and *that Air* muſt as *neceſſarily be rarefied* by the *Heat* of the *Warm Water*; and *that Rarefaction or Expanſion* will force the Mercury towards the Angle of the Syphon, where it has only the Preſſure of the External Air (thro' the *open ſhorter* Leg of the Syphon) to encounter with, as an *Impediment* to its *Motion that way*. Now, the *firſt Expanſions* of the *included Air*, by the *Heat*, are ſufficient to overcome the contrary Preſſure of the *External Atmoſphere:* And by this means the Column of Mercury is preſs'd towards the *Angle of the Syphon* ſo far, till the Rarefaction and the outward Preſſure come to balance one another. Then, as the Water *cools* (and the *Heat* growing *leſs*, the expanſive Force of the included Air by conſequence abates too) the Preſſure of the Atmoſphere thro' the *ſhorter* Leg of the Syphon begins to prevail, and conſequently forces the Mercury more inwards, or *farther from the Angle of the Syphon.* And thus the Rarefaction ſtill diminiſhing, and the Weight of the Atmoſphere gaining more and more upon it, the Mercury is ſtill driven farther from the Angle of the Syphon, and ſo the Length or Diſtance between it and the *End cover'd with the Cap*, becomes continually leſs and leſs. And *theſe Diſtances* are expreſs'd in *that Column of Parts* in the *Table*, which anſwer to the Degrees of the *Spirit*'s Deſcent in the *Thermometer.*

This Experiment it ſelf proves the Spring of the Air, as it ſhews us the *External* Atmoſpherick Column firſt giving way to the more *powerful* Expanſion of the *included* Air, and then by degrees *recovering it ſelf*, and forcing the Mercury away before it, towards the other End of the Tube.

We ſee likewiſe the Ground *gotten* by the *External* (which is the ſame with that *loſt* by the *included* Air) to anſwer exactly to the *Abatements of Heat* indicated by the *deſcending Spirits* in the *Thermometer:* So that the Liquor *there* always gave an exact and perfect Account of the *Contraction* or *ſhortening* of the *Column of included Air;* that is, of its *Denſity.*

The *abundant Uſefulneſs* of *which Obſervation*, I may ſome time or other more largely diſcourſe of.

EXPERIMENTS

Concerning the Refraction *of the* Air.

ABout Ten Years ſince, that Curious and Ingenious Member of the *Royal Society*, Mr. *John Lowthorp*, contriv'd an *Apparatus* to demonſtrate ſenſibly the *Refractions* of the *Air*, which hitherto had been perceiv'd only by the ſubtle and nice Diviſions of Aſtronomical Inſtruments. He made a *Vacuum* between two inclined Planes of Glaſs by the help of *Quickſilver*; thro' which an Object view'd with a *Teleſcope* was ſeen, upon re admiſſion of the Air, very ſenſibly to change Place: An Account of which Experiment is at large in *Phil. Tranſ.* N° 257, and in the *French Memoirs* for the Year 1700, to which I refer.

Mr. *Caſſini* the Son having been preſent when Mr. *Lowthorp* made this Experiment before the *Royal Society*, made a Report thereof to the *Royal Academy* of Sciences of *France*; and, upon his return home, thoſe *Scavans* thought it worth their while to re-examine the matter: But tho' themſelves thought it very ſtrange, yet, as they manag'd the Trial, they declare in the Hiſtory of their Academy for the Year 1700, That *it did not ſucceed*; and, that the Beams of Light paſſing thro' ſuch a *Vacuum* ſuffer'd no alteration by Refraction.

How well they made their *Vacuum*, tho' they ſay it was *bien exactement*, may juſtly be queſtion'd; or rather, the thing being ſo evident, it will not ſeem malicious, if we ſuppoſe ſome little Senſe of Emulation might incline them to deny the Honour of an Improvement of ſuch Conſequence to *Aſtronomy*, to a Foreign *Academy*.

The *Royal Society* (whoſe Glory it is to be as unwilling to deceive as to be deceived) being inform'd that this Experiment was call'd in queſtion by the *French Academy*, were deſirous that it might be put paſt diſpute, by repeated and fully atteſted Trials: Accordingly, I was order'd to make an Inſtrument for the purpoſe, by the direction of Dr. *Halley*, R. S. S. and Profeſſor of Geometry in *Oxford*. It conſiſted in a ſtrong Priſm of Braſs, two ſides of which had Sockets, to receive Glaſſes as truly plane and poliſh'd as could be gotten; and the third ſide had a Pipe with a Stop-cock, whereby to apply both the Exhauſting and Condenſing Engine. The Glaſſes were firmly fix'd and cemented in, ſo as to bear both an inward and outward Preſſure, and the whole turn'd upon an Axis, that it might be made to receive the Rays with any Obliquity deſir'd: And, to be the more ſecure, I affix'd a Mercurial Gage, to diſcover any the leaſt Defect in the Cement, that might happen; the Angle contain'd between the two Glaſs Planes being very near to 64 Degrees. And this Inſtrument, thus prepar'd, we fitted to a Teleſcope of about 10 Foot long, ſo as the Axis of the Teleſcope might paſs thro' the middle of the Priſm; and in the *Focus* of the *Teleſcope* a very fine Hair was adapted, for direction to the Sight.

Having choſen a proper and very diſtinct erect Object, whoſe diſtance was 2588 feet, *June* 15. *S V.* 1708, in the Morning, (the *Barometer* being then at 29. 7½, and the *Thermometer* at 60) we firſt exhauſted the Priſm, and then applying it to the *Teleſcope*, the Horizontal Hair in the *Focus* cover'd a Mark on our Object diſtinctly ſeen thro' the *Vacuum*, the two Glaſſes being equally inclin'd to the viſual Ray: Then admitting the Air into the Priſm, the Object was ſeen to riſe above the Hair gradually, as the Air entred, and in the end the Hair was found to hide a Mark 10¼ Inches below the former Mark. This often repeated, as often ſucceeded.

This done, we applied the Condenſing Engine to the Priſm, and having Pump'd in another Atmoſphere, ſo that the Denſity of the included Air was, by the Mercurial Gage, double to that of the outward; we again plac'd it before the *Teleſcope*, and then letting out the Air by the Cock, the Object, which before ſeem'd to riſe, now appear'd gradually to deſcend, and the Hair at length reſted on an Object higher than before by the ſame Interval of 10¼ Inches. And this likewiſe often repeated, never fail'd.

We again crouded in another Atmoſphere, and upon diſcharging the condens'd Air, the Object was ſeen near 21 Inches lower than the Hair; but in this, the great Preſſure forcing the Cement would not permit us to make ſo frequent Repetitions as in the former.

And theſe Experiments have been ſhewn before the Preſident; and, at times, to moſt of the Principal Members of the *Royal Society*. So that 'tis hoped, the Fact may no longer be queſtioned.

Now the *Radius* being 2588 Feet, Ten Inches and a quarter ſubtend an Angle of one minute and eight ſeconds; and the Incidence of the Viſual Ray being 32 Degrees, by reaſon the Angle of the Glaſs Planes was 64 Degrees, it follows, from the known Laws of Refraction, that as Sine of 32° to S. of 31°. 59′. 26″, ſo Sine of any other Incidence to the Sine of its refracted Angle; and ſo is *Radius*, or 1000000, to 999736; the Logarithm of which *Ratio* is ---- 0001145: whence the Refraction of the Air may readily be computed at any other Angle of Incidence.

By theſe Experiments it plainly appear'd, That the Refraction of the Air was, as far as the Eye could diſtinguiſh it, exactly proportion'd to its *Denſity*; the Refraction being the ſame from the common Air to a *Vacuum*, as from a *double Denſity* to the common Air, and the Refraction from a *treble Denſity* to the common Air exactly double to *that* from the common Air to a *Vacuum*. Whence the *Denſity* of the Air, in reſpect to the incumbent *Atmoſphere*, being always as the *Height* of the Mercury in the *Barometer*, the Refraction alſo will be, *cæteris paribus*, in the *direct proportion* of the *Heights* of the Mercury.

But this *Denſity* of the lower Air is conſiderably varied by *Heat* and *Cold*, as appears by the Table in Page 220; wherein we have ſhewn by *Experiment*, that the ſame Air which when the *Thermometer* mark'd 130 Degrees, (being the *greateſt Summer Heat*) occupied 144 Spaces, by *extremity* of Cold, or at 50 Degrees below the Freezing Point, was reduc'd to 126 of the ſame Spaces; but at the Freezing Point to 131: It being very remarkable, that the Air and Spirit of Wine did proportionably contract themſelves during the whole Experiment. Hence by help of the aforeſaid Table, we are enabl'd to give a Rule to eſtimate the *Refraction* of the Air at all times; having the *Height* of the *Barometer* and *Thermometer*: For with the ſame *Heat*, the Rarefaction is as the *Height* of the *Barometer* directly; and under the ſame Preſſure, it is as the Spaces of the ſame Air occupies reciprocally.

Now our *Experiment* being made when the Mercury was at 29,7$\frac{1}{2}$, and the *Thermometer* at 60, which gives the Space in the Table 137: Let it, for Example ſake, be requir'd to find what would be the Refraction when the *Barometer* is at 29 Inches, and the *Thermometer* at the Freezing Point, or the Air occupying but 131 Parts. I ſay, the *Denſity* of the Air at ſuch time will be to the *Denſity* at the time of our Obſervation, as 137 times 29 to 131 times 29,7$\frac{1}{2}$; that is, as 15892 to 15589; wherefore the Refraction of the Air, at ſuch time, will be in that ſame *Ratio* to what it was *June* 15, 1708. How to apply this Rule to Aſtronomical purpoſes, and how to correct the Errors occaſion'd by the Air's Refraction, in the Obſervations of the Stars, will ſhortly be ſet forth in a more proper place.

An

An Account of an Experiment concerning the different *Weights of the* ſame ſorts *of Bodies, but of very unequal Surfaces, in* Water, *which were of* equal *Weight in Common Air.*

IT is very well known, by many *Experiments*, That the *minute Parts* of Bodies, which are ſpecifically heavier than ſome *Menſtruums*, may, notwithanding their Exceſs of *Gravity*, be ſuſpended and held up therein.

This is ſeen in the *Diſſolution* of *Gold* in *Aqua Regia*, and of *Silver* in *Aqua Fortis*, and many other *Chymical Experiments* beſides.

Now theſe *Phænomena* have hitherto been us'd to be ſolv'd from the conſideration of the *great Increaſe of Superficies* (in ſmall Bodies) in proportion to their Bulk. For *theſe Metals* or other Bodies (ſay thoſe who go upon *this* Hypotheſis) being divided into *extreamly minute Parts*, by the Action of the *Menſtrua*, a vaſt *Increaſe of Superficies*, in proportion to the *Bulk* or *Weight*, is an immediate Conſequent thereupon.

And the Reſiſtance from the Liquid being *greater* or *leſs*, according to the Superficies, it comes to paſs that *Particles* of *Matter*, ſpecifically heavier than a Fluid propos'd, may by *that great Exceſs* of *Reſiſtance* above their *Gravity*, come to be *ſuſpended*, and *float* therein.

Now, from hence 'twas eaſie to infer, That if *this* was the reaſon of the *Phænomenon*, ſomething of this mighty difference muſt needs appear *by weighing equal Quantities of Matter*, and therefore *equally heavy*, but of very *unequal Superficies*, in *Water*, or ſome other *Liquid*; and then ſeeing how much the *one* exceeded the *other* in *Weight* there.

Accordingly, I took a Piece of *Sheet Braſs* of an exact *Inch ſquare*, and in Weight juſt 482 *Grains*. I then cut as many ſquare Inches of *Braſs Tinſel*, as were equal in *Weight* to the former, *viz.* 482 Grains; and theſe Pieces were 255 in *Number*. Now, here being ſo very great a difference of Superficies, I concluded, there would be ſome very conſiderable difference found, ariſing upon the weighing of *theſe Materials* in *Water*. But, to my great Surprize (being indeed, prepoſſeſs'd *in favour of the common Opinion*) *I found but two Grains difference*; the *ſingle Piece* weighing in the *Water* about 422 Grains, and the other *ſeparate* ones hardly two Grains leſs.

And this, upon two or three repeated Trials, made with all the Caution imaginable, *ſucceeded much the ſame*; ſo that the difference is not worth mentioning.

Now, here the Proportion of the Surfaces were as 1 to 255 (for I reckon the Sides of all the *Tinſel Laminæ* to be equal to the Sides of the *ſingle Braſs Lamina*) and notwithſtanding that in one caſe there was 254 times more Superficies, than in the other; yet there was ſcarce a $\frac{1}{211}$ part leſs Weight with all that Superficies, than with the former; which Decreement of Weight may, without ſcruple, be attributed to ſome ſmall Bubbles of Air, which adher'd to them unperceiv'd.

And from hence I am ſtrongly induc'd to conclude, that ſome other Cauſe muſt be found out to ſolve this Phænomenon by, ſince the Diſproportion between Superficies and Bulk or Weight of Matter, is not ſufficient to do it.

For ſuppoſe a ſmall *Metallick* Particle, or one of ſome other Body *ſpecifically heavier* than a Liquid: And, ſuppoſe this to ſink by its own Weight, if put into this Liquid. According to the foregoing Experiment, though this Particle were divided ſo, as to have 254 *times more Superficies* than now it has; yet its Loſs of Weight would be ſo inconſiderable, that no *ſuſpenſion in the Liquid were to be expected from thence.* And there is a pretty remarkable Confirmation of this to be drawn from an Experiment I made with the *Powder of Fine Flint* Glaſs. This Glaſs made uſe of, was of that ſort which is, of all others, the cleareſt and *freeſt* of *Blebs*. Farther: To have the other Parts of the Glaſs as *minute* as well might be, after it was reduc'd to *Powder*, I paſs'd it through a *Lawn Sieve*. And that there might be no Error ariſing for want of *a juſt Quantity of Matter*, to make the Trial with; I weigh'd an Ounce of this Fine Powder againſt the like Quantity of *ſolid Glaſs*.

And here likewiſe (as in the former Experiment) the Weight of this ſo finely powder'd Glaſs, in *Water*, differ'd by *ſuch a Trifle*, from the *Counterbalance* of the ſolid Piece in the *ſame Element*, that it was by no means worth taking notice of: Eſpecially too, ſince ſome parts of it *remain'd floating* in the Water, and never ſettl'd, or ſunk down into the *Bucket* of the *Hydroſtatical* Balance at all.

But what this way of arguing from the Greatneſs of the Surfaces of Bodies, and the *Reſiſtance by the Liquid ariſing* therefrom, will not do, (with reſpect to the accounting for *that* Suſpenſion in a *ſpecifically lighter* Medium) I believe, may be done by another Method, and that *effectually*. In ſhort, the ſuſpenſion of the heavier *Particles* of *Matter* in Liquids, I attribute to the *ſame Cauſe that keeps the Liquors ſuſpended in ſmall Tubes*: I mean, *Attraction*.

The *minute Parts* of Bodies conſiſting of *Plane* Surfaces, *being ſtrongly attracted by the Parts of a Fluid, in which they are plac'd* (and therefore reciprocally attracting the *Parts* of *that* Fluid again) *may, by the Action of theſe Forces, be held ſuſpended therein.* And what *little* Bodies are not, or will not be, *held ſuſpended* in a Liquid, but are *let fall* therein to the Bottom of the containing Veſſel, I believe to be ſo, upon one of theſe *two Accounts:* Either that the Parts of the Liquid *do more ſtrongly attract one another*, than they do thoſe *little Bodies interſpers'd* among them (which therefore *ſubſide* upon *that* ſcore;) or elſe, that they do by their *own Attractions* form themſelves into *little Cluſters*, whoſe Bulk and *ſuperiour Momentum* help to *precipitate* them downwards.

This being laid down as the true Cauſe of the *Suſpenſion* of ſmall ponderous Particles of *Matter* in Liquids; I believe, our common Notions of *Corroſion* and *Diſſolution*, may alſo be rectified from the ſame Principles. A Corroſive Liquor, or Diſſolvent, in the vulgar ſenſe, is a very unintelligible thing. For, not to mention *other Incongruities*, 'tis not to be conceiv'd, what ſhould *carry the Parts of a Liquid with ſo prodigious an Impetus, into the Pores of a ſolid Body*, ſo as to diſſolve the whole Texture of it, and reduce it into *inſenſibly ſmall Parts*. But an *Attractive* Force in that ſolid Body will do this; by which the Particles of the Fluid are forced into the Interſtices of it, *with a Momentum*

tum ſuperiour to that of the Coheſion of its Parts. For, this ſuppos'd, its Parts will be ſeparated from one another; that is, the Body will be *diſſolv'd.*

But the time, perhaps, will come, when this *wonderful Law of Attraction* (as it obtains in the ſmaller Portions of *Matter)* will be more fully and clearly underſtood, and ſome new Effects of it diſcover'd, which now are not ſuſpected to proceed from *that* Cauſe.

AN

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APPENDIX,

Containing ſome General Remarks *on ſome of the foregoing Experiments.*

THO' there are none of the Experiments related in the *foregoing Tract*, but what will, I hope, be of ſome uſe to the *Intelligent Philoſophical* Reader, (ſerving at leaſt to excite him to make farther Improvements himſelf, in *Experimental Knowledge*, if they don't give him all the Information he needs or deſires ;) yet there are ſome of them which being, I think, *quite new*, and moreover *very ſurprizing*, I thought it might not be amiſs to fill up a few Pages here with ſome Enquiries into, and Reaſonings upon them.

The Experiments I *principally* refer to, are thoſe of *Electricity* and *Light* produc'd by *Attrition*; of which the Reader may find a large Account from *pag.* 24. to *pag.* 87. and theſe relating to *various ſorts* of Bodies, and in various *Mediums* too.

I begin with the Phænomena of *Electricity.*

There are ſome of theſe ſo ſtrange in their Circumſtances, that I confeſs, I am apt to think there are *not many in nature*, more ſurprizing than they are.

But tho' the Diſcovery is yet but *young*, and has not been made long enough, to be *throughly and perfectly diſcuſs'd*; yet ſome things which are either *plain* and *certain*, or probable and likely, may be advanc'd in the mean time; and as all Circumſtances and Varieties in thoſe Experiments come to be more accurately examin'd, 'tis to be hop'd we may arrive at more *Poſitive Concluſions*, about *the Reaſons of* the Phænomena.

The Four following Propoſitions relate to the Attrition of Tubes.

Prop. 1. *Within the Body of the Glaſs, are contain'd and lodg'd certain Parts of Matter, of conſiderable Force and Activity, which by their Motions and Percuſſions are the Cauſes of all theſe Effects.*

That there is an Emiſſion of ſome Matter conſequent on the Friction, I think is too plain to be queſtion'd; for 'tis obvious almoſt to every one of our Senſes: To the *Eye*; by the Motions of the Leaf-Braſs, and by the Light produc'd, when the Tube was rubb'd in the Dark: To the *Feeling*; by the ſenſible ſtrokes and puſhes, made upon the Face when the Tube was held near it; To the *Ear*; by the *Noiſe* and Crackings, the Eruption was accompanied with, which might be heard at the diſtance of Seven or Eight Foot.

That this Matter emitted, is alſo emitted from or by the Tube; I take to be as plain as the former. For how elſe ſhould the rubbing of the Tube, ever be an occaſion of this Matter's diſplaying and exerting it ſelf? If it come not from thence, the Attrition of the Tube could not fetch it from any other Body diſtinct from the Tube; but the Teſtimony of Sence aſſures us of this Likewiſe: For all the Motions of the Leaf-Braſs are directed to, or from, or about the Tube; and therefore 'tis beyond all diſpute, *that the Courſe of the moving Matter is from thence*. And I believe, there's hardly any one but will allow, that this Matter, if it *came from the Tube was certainly repos'd and lodg'd there before.*

Prop. 2. *The Motion of this Matter is not equable and regular, but diſorderly, fluctuating and irregular.*

This appears from part of the Fact related. For the little Bodies ſometimes would be drawn to, ſometimes thrown from the Tube with violence, ſometimes be ſuſpended for a ſmall time in the Air, and at other times ſlip along the ſides of the Tube. They would repeat theſe Leaps and Boundings for ſeveral times together, and flutter up and down almoſt like ſo many Animals, rather than pieces of Lifeleſs Matter. Now this Variety cannot be the Effect of any even and regular Motion. It plainly ſhews the moving Force to exert it ſelf (as it were) by fits; and to be *propagated every way about in a confus'd irregular Orb.* For if Bodies once put in motion, can't of themſelves alter their Direction, but are over-rul'd by a foreign Force whenever they do it; and if the Diverſities of their Motions muſt needs infer juſt as great a diverſity in the Impulſes of the Bodies that move them; then ſince our pieces of Leaf-Braſs, in this Experiment, were ſo very odd and extravagant in their Motions, 'tis plain, that the Effluvia (which alone can be the moving Bodies here) muſt themſelves alſo be hurried *after a very irregular manner.*

Prop. 3. *The Air contiguous to the inner Surface of the hollow Tube, has an Influence on Operations of the Effluvia.*

This plainly follows, becauſe when the Tube was exhauſted, and the *contain'd Air drawn out*, the Leaf-Braſs would ſcarce be ſtirr'd at all, tho' with a much more forcible Attrition, and at a much leſs diſtance, than when the Tube was full of Air. And beſides, when the Air was let into the Tube again, the attractive Power (which was before almoſt loſt) was ſtrangely and ſuddenly recover'd again. Which is an undeniable Proof, that the Preſence of that contiguous Air did ſome way or other contribute to the more powerful and effectual Operation of the Effluvia. Neither is it an Objection of any moment againſt this, that the Attraction is as powerful in the *caſe of the ſolid Tube*; where there being no *Cavity*, there can conſequently be no *contiguous Air*. For this only proves that there is as ſtrong an Attraction in a ſolid Tube, as in a hollow one; but it does not prove, that the Air was of no Advantage in the Caſe of the hollow Tube. *To prove that an Effect may be the ſame, in two very different Circumſtances; is not the ſame thing as to prove that it has no manner of relation to this or that particular Cauſe in one of thoſe Circumſtances.* And therefore to argue from the Leaf-Braſs being ſtirr'd as vigorouſly by the Effluvia, when the ſolid Tube was uſed; will not be ſufficient to ſhew that the Air has no manner of influence in the Circumſtance of the hollow Tube.

For the Propoſition does not aſſert, that the Effluvia can in no Caſe exert themſelves with vigour, without the concurrent Aſſiſtance of the Air: But it aſſerts, *that the contiguous Air had ſome advantageous Influence in the Caſe of the hollow Tube.* And this is as evident, as that the Air is ſerviceable to the vital Functions of Animals, or that they cannot live and breathe without it. For as upon the depriving an Animal of the Benefit of this Element, *all the Powers flag; the Springs of Motion become feeble and drooping; and at laſt ſink away into a fatal inactivity:* So here, if the Tube be exhauſted of Air, *the Effluvia loſe all that briskneſs* which wrought ſuch ſurprizing Effects before; and continue, as it were, impotent and dead, till a freſh return of Air inſpires them again.

And to add a Demonſtration of the Air's power, with reſpect to the Operation of the Effluvia, which will, I think, admit of no Exception; I deſire it may be conſider'd, that the Effluvia will not be excited by any Friction, to produce any Effects, if the Attrition of the Tube be made in *Vacuo*: And that, whether it be a cloſed *hollow* Tube replete with Air, or even a ſolid Tube it ſelf: To either of which, I can give what degree of Friction ſoever is neceſſary, in an exhauſted Receiver. The contiguous Air, I ſay, being removed, the Electrical force ſeem'd to be quite gone; and continued ſo to be, till the Preſence of the Air was reſtor'd. Now this is a plain Proof of the neceſſity of the Air, to the Operations of this Attractive Matter. Wherein that neceſſity lies, or what aſſiſtance 'tis which the Air contributes, I don't here determine; but that the thing is ſo, *is ſo manifeſt, that I cannot expect to ſee any thing more clearly prov'd by Experiment than this is.*

Prop. 4. *It does not ſeem that the Air included in the Cavity of the Tube, can have any Influence (with reſpect to the Action of the Effluvia) but one of theſe two ways:* Either, by the *forcible Endeavour of its Spring* againſt the *contiguous* Body of Glaſs, helping to puſh and impell that *active Matter* outwards, which is already prepar'd and diſpos'd by the *Attrition* for ſuch a *Remove*; or elſe, as (by virtue of the *ſame Principle*) it *hinders the Electrical Matter from retiring inwards*, by acting as an *Impediment againſt* it; and ſo only *occaſionally* cauſes the more ſenſible and remarkable Effects of that Matter, upon little Bodies plac'd in its way *without* the Tube.

It may be, that the *Heat* produc'd by the *Motion* and *vigorous Attrition* of the Glaſs, may produce ſome *degree of Rarefaction* in the Air contiguous to the *convex* or *outward Superficies*. And then in that *Caſe*, there being not the like Rarefaction in the Air contiguous to the *concave* or *inner* Surface, (for the *rubbing* cannot produce that Heat upon a diſtant Surface, that it does on that which is *immediately* rubb'd;) the Electrical Matter will, with *much more difficulty retire in* towards the Cavity of the Tube, than it *will go outwards* : Becauſe the *Equilibrium being loſt on the Outſide*, it will neceſſarily be carried that way where it meets with the leaſt Oppoſition. And certainly, the Spring of the *leſs rarefied* Air *within*, is ſuperiour to the *Preſſure* of the *more rarefied* Air *without*.

And therefore, on the other hand, *when the Tube is exhauſted of its Air*, and conſequently the *Balance* loſt on the *Inſide*; all the Attrition that can be given, will not be ſufficient to *bring the Effluvia out* againſt an *incumbent Preſſure*, as long as the inward Cavity is *clear* of Air, and there is no *Counter-force* to oppoſe their *Conatus* or *Tendency* that way.

Prop. 5. *As the* internal *Air is neceſſary to the Action of the Effluvia*, ſo is the *external too* : Becauſe, tho' the Tube was very full of Air, yet if rubb'd *in Vacuo*, the Attractive Power was quite loſt.

Prop. 6. *As therefore the* internal *Air ſeems neceſſary, either to aſſiſt the Electrical Matter in its Motion outwards, or at leaſt, to prevent its retiring inwards ; ſo the* external *Air appears to be as neceſſary to carry the little Bodies* (which we ſay are attracted) *towards the Tube.*

For if by the Heat and Rarefaction, conſequent upon the Attrition, *the Medium contiguous to the Tube*, be made *ſpecifically lighter*; then of courſe, *to keep up the Balance, the remoter Air*, which is *denſer*, muſt preſs in towards the Tube, and ſo carry away (in the Torrent) the little Bodies lying in its way, thither alſo.

Prop. 7. The *various Irregularities* in the *Excitation, or the Emiſſion and Diſcharge, of the Electrical Matter from the Tube* (which will be follow'd with proportional Irregularities, in the Motion and *Tendency of the denſer Air*, towards the Tube, by the *Hydroſtatical* Laws) *may be ſufficient to account for the various uncertain Motions of the little Bodies carried towards the Tube.*

I ſhall now add ſomething concerning the Effects of the Electricity of the Glaſs *Globe* and *Cylinder.*

Prop. 1. The *Preſence of the Air* is *neceſſary* to this *Phænomenon, of the regular Direction of the Threads*; as well as to *that* of the *Attraction of the Tube.*

Becauſe, if the Semi-circular Hoop of Threads were plac'd *in Vacuo*, that Property of their regular Direction to a Center would be quite loſt, even tho' the *Globe* or *Cylinder* were *full of Air.*

Prop 2. The reaſon therefore, *why the Threads are not directed in this caſe*, does not ſeem to be, *becauſe there is no Electrical Matter* diſcharg'd from the Glaſs (by the Attrition) to draw and direct them thither; but becauſe there *wants a current of External Air*, to put them into the aforeſaid *central Direction.*

For the *extrnal* Air being *abſent*, and the *internal preſent*; The Matter ſhould find a vaſtly eaſier paſſage outwards, than inwards; and therefore ought to be diſcharg'd that way. But then, becauſe the external Air is remov'd, there is *no room for the loſs and recovery of an Equilibrium* to take place; and conſequently no *Flux* of a *Circumjacent Medium* that way, and ſo, *no Direction of the Threads*. For,

Prop. 3. *If the Electrical Matter be emitted* in *Phyſical Lines, every where diverging from the Center of that Circle in which the Attrition is made* (or in the *Plane* of which the *Hoop of Threads* ſtands) *towards the Circumference of the ſame Circle*; then by the *Rarefaction* of the *Medium contiguous to the Glaſs*, and the *neceſſary Preſſure of the more remote and denſe Medium*, into the Plane of that ſame Circle, with *Directions contrary* to thoſe in which the Effluvia are emitted: *By this means*, I ſay, the Threads may be regularly directed to the Center

ter of that Circle in whoſe *Plane* the Hoop to which they are fix'd, is plac'd.

For the *Flux of the denſe Medium* will be in *Directions contrary* to thoſe according to which the *Rarefaction* is made. But the Effluvia are (by the *Hypotheſis*) emitted in *Phyſical* Lines, *diverging from the Center towards the Circumference.* Therefore the *Rarefaction* of the *adjacent Medium* is according to the *ſame* Directions. And therefore the *Flux* of the *remote denſer* Medium, is in Lines *converging* from the *Circumference* towards the *Center.* And all this (by the *Hypotheſis*) being in the *Plane of Attrition*; that is, in the Plane, *wherein the Hoop of Threads ſtands:* Therefore the Threads are in the ſame Plane, wherein the Flux of the denſe Medium paſſes *in Lines converging from the Circumference towards the Center.* And therefore by *the Action of the ſaid Medium* the *Threads may be forc'd into a regular Central Direction.*

Prop. 4. For the ſame reaſon; If the *Plane of Attrition be different from that Plane wherein the Threads are fix'd*; the Threads ought to form themſelves in a ſort of *conical* Surface; or rather the Surface of a *Trunk* of a *Cone*, whoſe *Vertex* would be ſome point in the *Axis* of the *Globe* or *Cylinder*; were the *diſcharge of Electrical Matter* every way *equable* and *uniform.* And we find it matter of Fact, that the Threads *did actually form themſelves into this ſort of Figure.*

So that if there were two Hoops of Threads, plac'd one on one ſide, and t'other of the other ſide the *Plane of Attrition*, there would be two *Curti cone* Surfaces form'd; of which the *more Acute* would be that which is fartheſt from the Plane of Attrition, and the *more Obtuſe* that which is *neareſt* thereto. For when the *Plane of Attrition*, and the *Plane wherein the Threads are plac'd*, do co-incide; then the *Conic Surface* is chang'd into the *Area of a Circle:* Becauſe then the Threads lie all in *one and the ſame Plane.*

Thus much concerning the *Electricity.* I would now ſubjoin ſome few Things concerning the *Lights* produc'd in theſe Experiments.

Prop. 1. *Tho' the Electrical Quality neceſſarily requir'd the preſence both of the External and Internal Air in order to its ſhewing it ſelf;* yet the *Light* requir'd the preſence but of *one* of 'em, *viz.* either the *inward* or the *outward* Air, in order to its Appearance.

For either a Glaſs Globe *full of Air*, rubb'd in *Vacuo*, or with its Air *exhauſted*, and rubb'd in *Pleno*, would *either way* produce a very *conſiderable Light.*

Prop. 2. There ſeems therefore to be *a real difference between the Electrical and Luminous Effluvia* (at leaſt in *ſome* Caſes:) For by the foregoing *Prop.* theſe Qualities require *different Circumſtances* with reſpect to the *Circumjacent Medium*, in order to their diſcovering themſelves. And more than that; a *ſtronger Attrition*, which generally heightens the Effects of the *Electricity*, does not at all *contribute to the Increaſe of the Light.*

Nay, *Light is producible by the Effluvia of one Glaſs falling on another*; but the *Electrical Matter* is not to be brought forth by any ſuch Feeble Strokes or Impulſes as thoſe are.

Prop. 3. *Thoſe* Lights (in ſome Circumſtances at leaſt) *are leſs ſenſibly affected by the Return of the Air, which are produced upon an Attrition of* exhauſted Glaſs *in* Pleno, *than thoſe produc'd by the Attrition of* Glaſs *full of* Air *in* Vacuo.

For in the former Caſe, no great Alteration was found in the *Light* or *Colour*, till a *certain Quantity of Air* was let into the Inſide of the exhauſted Glaſs. But in the latter Caſe, both *Light* and *Colour* were ſenſibly changed, at every Admiſſion of Air, on the Outſide of the *Full Glaſs.*

Prop. 4. *Of the various Lights produc'd from various Bodies by Attrition: or* (which is equivalent thereto) *the Concuſſion and Agitation of their* Parts; ſome *are much more confin'd to a* particular Medium, *as a* neceſſary *Condition of their Appearance*, *than* others *are.*

That of Culinary Fire, is abſolutely limited to ſuch a Medium as Common Air.

Thoſe of Amber, Woollen, Oyſter-ſhells, &c. require a *Vacuum*, or the neareſt Approach to it, and utterly diſappear in a Groſſer Medium.

The Mercurial Lights are yet more unlimited, as to the Condition of the Medium in which they appear.

For, as they are producible in *Vacuo*, and in a rarefy'd Medium approaching thereto; ſo I have alſo ſhewn, That a Light of this kind may be made to appear even in Common Air it ſelf.

And thus much for the *Phænomena* of *Electricity* and *Light*, produc'd by Attrition.

From all put together, I hope, ſomething may ariſe, that may be ſerviceable to the Deſign of gaining ſome true Knowledge of the Cauſes of ſo ſurprizing Appearances.

And if any one ſhould luckily Improve theſe Short Hints for that purpoſe, I ſhall have obtain'd my End.

BRITISH MUSEUM 30 NOV 67

A
SUPPLEMENT.

The following Experiments being made by Mr. *Haukesbee* after the Publication of the Firſt Edition of this Book, and not rang'd by him under any proper Section before his Death; we have here printed them juſt in the Order he deliver'd them to the Royal Society.

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Experiment I.

An Account of an Experiment touching the different Denſities of Common Water, from the greateſt Degree of Heat in our Climate, to the Freezing Point, obſerved by a Thermometer.

I Caus'd a Quart of Water to be heated near ſcalding hot, and then put it into a convenient Glaſs with my Thermometer, the Spirit in which ſoon aroſe into the Ball a-top, where it remain'd, till the Water cooling, caus'd it to deſcend: By this time the Spirit in the Thermometer and the Water were become of an equal Temperature; and when it had deſcended to 130 Degrees above the Freezing Point, I began my Obſervations; which take as follows:

I weigh'd a ſmall Bottle in't, and found the Bulk of Water equal to it in that ſtate, was 574 Grains.

When the Spirit had deſcended to 80 Degrees above the Freezing Point, the Bulk of

Water equal to the Bottle, then weighed three quarters of a Grain more than before.

At thirty Degrees above the Freezing Point, the Quantity of Water equal to the Bulk of the Bottle, was again increas'd about three quarters of a Grain.

At the Freezing Point it weigh'd ſtill ſomething more; in all, about two Grains, from 130 Degrees above the Freezing Point, to that very Point.

Which to me ſeems conſiderable, and ought to be taken notice of by ſuch Gentlemen, who judge of a Mineral, or any other Water, by its Weight, when they have not an opportunity of making the Experiment at the Fountain-head: For there, I ſuppoſe, the Water is at the ſame Degree of Temperature at all Seaſons.

Now, according to this Experiment, I find, that Water is condenſible by Cold one 28th Part of the Whole, from the greateſt Degree of Heat in this Climate. Suppoſing then, that the Water in the Sea ſhould ſuffer the ſame Alterations by the Change of the different Seaſons (as I ſee no reaſon but very nearly it muſt) it would be eaſie to compute, that a Ship which ſhould draw two Fathoms, or 12 Feet Water, in ſuch Weather as is underſtood by the *greateſt* Degree of *Heat*, would draw about half an Inch leſs from the *greater Denſity* of the Fluid, when reduc'd to the premention'd Degree of Cold; and conſequently would Sail *better* at *that time.*

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But this is not all that occaſion'd the making this *Experiment*; for I did it in order to another. And ſince I find, that Water is capable of Dilation and Contraction by *Heat* and *Cold*, I ſee no reaſon why the ſame may not be perform'd by Force, notwithſtanding the many *Attempts* to determine it, have as yet been *fruitleſs*.

For ſince the *conſtituent Parts* of the Fluid, are capable of being remov'd at greater Diſtances one from the other by *Heat*, and become more cloſely united by *Cold*; ſo I conclude, that there muſt be ſome Body contain'd in't of an *Elaſtick Quality*, which I think, muſt be ſubject to the ſame Laws of ſuch a Body; that is, be capable of Compreſſion by force, as well as to become more Denſe by Cold. But the Iſſue of this I muſt leave to a more proper Seaſon.

Experiment II.

An Account of ſome Experiments, in relation to the Weight of Common Water, under different Circumſtances.

FIrſt, I took a Glaſs of Common Water; and having weigh'd nicely a *Glaſs-Bottle* in it, whoſe Bulk was equal to the Bulk of 575 Grains of the ſame Fluid: Then I caus'd ſome of the ſame Water to be boyl'd over the Fire; and after that, it was included in *Vacuo*, and there remain'd till it became of the ſame Temperature (as to Coldneſs) with Common Water.

Thus to the utmoſt of my power, I endeavour'd to *Extricate* all the Air out of the *Water*, thinking in *that* ſtate it would become more denſe than when I weigh'd my *Bottle* firſt in it: But contrary to my Expectation, I found that the *Bottle* had juſt the ſame Weight in it as before: Which ſeems to confirm the Impoſſibility to compreſs Water by force into a leſſer ſpace than it naturally poſſeſſes.

For if upon the Removal of ſuch a Quantity of Air from out of its Body, the Parts do not ſlide any cloſer together; how ſhould a Weight laid upon its Surface, when its Interſtices ſeem to be replete with Air, make any Impreſſion on it?

The Body which is forced out of the Water, by the premention'd means, I call Air; ſince for any thing to the contrary that I can diſcover, it is ſubject to all the ſame Laws with it: But that the Water, upon its Abſence, ſhould not unite more cloſely than before, ſeems very ſurprizing to me: For I cannot conceive what Matter muſt ſupply the Vacancies, ſince the Particles of Water themſelves remain at the ſame diſtances as if the Air was not withdrawn; otherwiſe the Water of neceſſity muſt become more denſe.

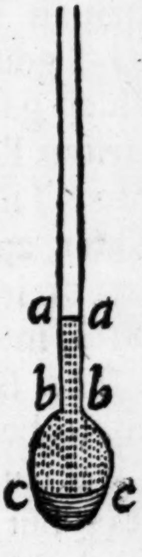
But, to proceed; I caus'd ſome Water to be heated about Blood-warm: When weighing my *Bottle* in it, I found the Bulk of Water equal to the Bulk of the *Bottle*, was about Three Grains leſs than when cold: Which ſhews, that the Component Parts of the Water are eaſily ſeparated by Heat, and the Matter lodg'd in its Interſtices, capable of Dilation.

Then I took that Water that I had purged of all its Air (as near as I could) and gave it a Degree of Heat, not exceeding Lukewarm: Upon weighing the premention'd Bottle in it, I found, that although the Heat it had received was very inconſiderable; yet the Bulk of the Water, in that ſtate, equal to that of the Bottle, was now diminiſh'd Two Grains: Which plainly ſhews, that notwithſtanding the Water contained no Air that I could diſcover; yet there ſeems a Matter latent in it, capable of Intumeſcence.

Experiment III.

Touching the Freezing of Common Water, and Water purged of Air.

THIS Experiment was recommended to me, in order to diſcover what difference would happen in the Swelling or Bulk of Ice, producible on the Freezing of Common Water, and Water purged of Air. Accordingly, I procured a couple of Glaſſes, in form of the Figure in the Margin. Theſe, when fill'd with the different Waters to a determinate Height, ſuppoſing at *a a*, I convey'd into the Freezing Mixture (which was nothing elſe but a Compoſition of Snow and Bay-Salt powder'd pretty fine) where they did not remain above three or four Minutes of time, before the Congelation began in each of them, which was very diſcernible, by the Aſcent of the Water in their reſpective Tubes, above their firſt Heights *a a*; and in about an Hours time, it had aſcended in that Glaſs, which contained the Water purg'd of Air, at leaſt



ſix Inches; but in the other Glaſs with common Water, not ſo much by more than an Inch; there being ſuch a Diſparity in the Content of the two Glaſſes; the laſt mention'd being leſs by a fifth part than the other, which contain'd not full four Ounces.

It was obſervable, that during the Glaſſes continuing in the Frigorifick Mixture, ſmall Bubbles of Air did continually aſcend in that which was fill'd with common Water; but not the leaſt Sign of any ſuch Appearance in the other.

When I had taken them out of the premention'd Mixture (which was at ſomething more than an Hours time from their firſt putting in) I pour'd from them the Unfrozen Water, which gave me the liberty of diſcovering the various Forms the new made Ice had ſhot it ſelf into; That Glaſs which contain'd the purg'd Water, appearing all over the Sides and upper part of it, to the very Neck *b b*, of divers Figures, much reſembling thoſe of Salts.

The Bottom Part of it *c c*, diſcover'd it ſelf to be ſeemingly ſolid; but whitiſh, as if it was full of very minute interſperſt Vacuities; but not like thoſe *Cavities*, which are very obſervable in the Freezing of *Common Water*: And, what was very notable, at the *Bottom* of the *other* Glaſs, they appear'd in great Numbers, of a longiſh Form, ſeemingly pointing all round from the Circumference to the Center of it.

There were none of thoſe *prementioned Salt-like Figures* on the Sides of *this*, as the *other*; but it was almoſt clear from any Adherence of Ice, ſaving towards the *upper part* near the Neck, where a *little* had faſten'd it ſelf with *thoſe* longiſh Bubbles, pointing from *that Part* downwards, inclining to the *Center.*

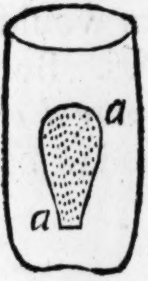
From all which I cannot but conclude, That the *Ice* produc'd from the *Water* purg'd of Air, was *equally augmented* in its Bulk to the *Quantity of Water* from which it was produc'd, as *that* which proceeded from the *Frozen Common Water*: For had the Glaſſes been of an *equal Content*, I ſee no reaſon to doubt, but the *Water* would have been equally Frozen in *both*, and the *Aſcent* of the *unfrozen Part* of 'em would have been much the ſame in their Tubes. But if there be any difference, the *Water purg'd of Air* ſeems to claim the eaſieſt Diſpoſition to be Frozen.

The Water I purg'd from the Air in the following Manner:

I firſt boil'd it well over the Fire: Afterwards I included it in *Vacuo*; where it remain'd in *that State* till it was cold: From whence I took it, and proceeded preſently on the *Experiment*, which on Two Trials ſucceeded alike.

Experiment IV.

An Account of an Experiment touching the Freezing of Common Water, Tinged with a Liquid ſaid to be extracted from Shell-Lac.



THIS Liquid is a very deep Red; a ſmall Quantity of which, will tinge twenty times as much of Common Water of a very good Sanguine Colour hardly Tranſparent. I found this Liquor, extracted from *Lac*, would not Freeze; for during the Coldeſt Weather we have lately had it retain'd its Fluidity; and when it was mix'd with Water, and expos'd to Freeze, the Water, in which it was mix'd ſoon ſuffer'd a Congelation; and ſo much of it as underwent the Change appear'd of a fine but pale Tranſparent Red; the Body of the Colour retiring into the Middle, in form of the Figure *a a*, in the Margin, and was wholly Opaque. And when no more of the mix'd Liquid would be Frozen, I took the Body of Ice out of the Glaſs that contain'd it, by juſt warming the Sides of it by a Fire. I found then by pricking a piece of Wire into the dark Part of it, the Red Liquor immediately ſucceeded thro' the Hole I had made, ſeemingly as pure and as abſtracted from any Mixture of Water, as it was before it was put into it. This Red Liquor I found to be ſomething ſpecifically heavier than Common Water; which makes me wonder, why the Figure it made on its retiring, was not rather the reverſe to what it appear'd: For I ſhould thing it reaſonable to expect, that the upper Part, which was the broadeſt, ſhould, by its own weight, alter or reverſe the Poſition of the Figure. Another thing very remarkable, was, that this retir'd Liquid, as it ſeem'd to keep an equal diſtance from the Sides of the Glaſs, ſo did it from the Bottom and Top of it; which upon repeated Trials anſwer'd the ſame.

I likewiſe mix'd ſome Common Water with a ſtrong Purple Liquor, made from Logwood, boyl'd in Water, in which ſome Allom had been diſſolv'd. A little of this would give a ſtrong Tincture to a pretty Quantity of Fair Water; and when expos'd to Freeze, would retire towards the Middle, leaving the firſt Frozen Water of a very pale Purple, in compariſon to the Middle part; which when I had taken out of the Glaſs that contain'd it, and broke it, I found 'twas Frozen through, but of ſo dark a Colour in the Middle, that it came near a Black.

Experiment V.

An Account of an Experiment, ſhewing that actual Sound is not to be tranſmitted through a Vacuum.

THAT the Experiments already made, endeavouring to produce a Sound from a Bell *in Vacuo*, have beeen altogether Ineffectual, is ſufficiently manifeſt: Yet that the loſs of it ſhould be wholly attributed to the Abſcence of the Air, I think could not without another Experiment be abſolutely concluded, ſince the following Query, (which very well deſerves an Anſwer) might fairly be ſtarted upon this Occaſion.

Whether the Sonorous Body in ſuch a *Medium* might not ſo ſuffer, or undergo ſuch a Change in its Parts, as to be render'd uncapable of being put into ſuch a Motion as is requiſite for the Action or Production of Sound.

Now to ſet the Matter of Fact in a true Light as near as I could, I contriv'd the following Experiment.

I took a ſtrong Receiver, arm'd with a Braſs Hoop at bottom, in which I included a Bell as large as well it could contain. This Receiver I ſcrew'd ſtrongly down to a Braſs-Plate, with a wet Leather between, and was full of Common Air, which could no ways make its eſcape. Thus ſecur'd, it was ſet on the Pump, where it was cover'd with another large Receiver. In this manner, the Air contain'd between the outward and inward Receivers was exhauſted.

Now here I was ſure, when the Clapper ſhould be made to ſtrike the Bell, there would be actual Sound produc'd in the inward Receiver, the Air of which was of the ſame denſity with common Air; and could ſuffer no Alteration by the *Vacuum* on its outſide, ſo ſtrongly was it ſecur'd on all ſides. And as I ſaid before, that if the Sonorous Body ſhould ſuffer in any meaſure, by being in a very rare *Medium*, ſo as to contribute to the loſs of its Sound, that this Method ſeem'd probable to diſcover it.

Thus all being ready for Trial, the Clapper was made to ſtrike the Bell; but I found that there was no tranſmiſſion of it thro' the *Vacuum*, tho' I was ſure there was actual Sound produc'd in the Receiver.

This plainly ſhews, and ſeems poſitively to confirm, That Air is the only *Medium* for the Propagation of Sound.

Experi-

Experiment VI.

An Account of an Experiment, touching the Propagation of Sound, *paſſing from the Sonorous Body into the Common Air, in one Direction only.*

SInce by the *Former Experiment* actual Sound could not be *tranſmitted* through a Space void of Air; I was inclin'd to try, whether *that Sound*, which ſhould be propagated in a Receiver, having a Communication with the Open Air at one *ſmall Aperture* only, but otherwiſe intirely ſurrounded by a *Vacuum*; whether, I ſay, that Sound would be increas'd, or continu'd Sounding longer, at each Stroke that ſhould be given the Bell, than it would do, were not its Body encompaſs'd by ſuch a *Medium*. In order thereto, the Bell was included as in the *premention'd Experiment*; only to the upper Part of its Receiver was ſcrew'd a Box with Collars of Leather; and on the Top of the outward Receiver, was laid a Braſs Plate with a wet Leather between: In the middle of which Plate, was likewiſe ſcrew'd another Braſs Box with Collars, as before.

Theſe Receivers, when plac'd on the Pump, had their Boxes ſtanding directly one over the other. Thro' both of them in *that Poſition*, I paſs'd a hollow Braſs Tube, which *exactly fitted* their *Perforations*: Thus the inward Receiver had a Communication with the outward Air, and the outward Receiver thereby was ſecur'd from the Ingreſs of the *Circumambient Medium*. Now, when the Air contained between the Receivers, was pretty well exhauſted, and the Bell ſtruck, the Sound was ſenſibly very vigorous, and (I think) very nearly as great as before any Air was taken away at all; yet if one's Finger was apply'd to the *Aperture* of the hollow Braſs Tube, the Sound would be ſo much diminiſh'd, as but juſt to be diſtinguiſhed. By this we ſee, that ſince the Sound in *that State* cannot be tranſmitted through the Receiver that includes it, by means of the ſurrounding *Vacuum*; yet the Receiver is certainly ſtruck with it; but finding no conveyance that way, reverberates, and makes its Paſſage where it finds leaſt reſiſtance. Nor did I obſerve, that altho' the Sound had but one Paſſage from its Receiver, and that but a ſmall one, that it continu'd any longer from the Stroke, than if it had been made in the open Air.

Experiment VII.

An Account of an Experiment touching the Propagation of Sound through Water.

AN Experiment that I made ſome time ſince, ſhewing that Actual Sound could not be *tranſmitted* thro' a *Vacuum*, gave me an Inclination to try what would be the effect, to ſurround the Receiver that contain'd the ſounding Body, with ſo denſe a *Medium* as *Water*.

Accordingly, as in the *former Experiment*, the Receiver which contain'd the Bell, was ſcrew'd down to a Braſs Plate, with a Leather between; This Receiver with its Bell, was ſuſpended in a large Glaſs Veſſel, by four Twine-Threads to the Top, and as many to the Bottom: Whereby it remain'd in the Middle between both; concluding likewiſe, That theſe Threads would abſorb the Water when it ſhould come to be put in, that there could be no Apprehenſion that any Sound ſhould be convey'd by them from the ſounding Body, any more than if they were intirely Water.

Thus provided, the Clapper was made to ſtrike the Bell, whoſe Sound was ſomething leſs by the Interpoſition of the Glaſs, than it would be, had it been made in the open Air: However, it was very audible, and might be heard at a conſiderable diſtance. It appear'd to the Ear to be very harſh, in reſpect to the Tone it afforded us. But now, when the Water came to be pour'd in, and the inward Receiver ſurrounded by it, at leaſt an Inch and half from the neareſt Part of the outward Glaſs, the Clapper was again made to give the Sound; which it did, ſeemingly, very little leſs, in reſpect to its *Audibility*; but much more mellow, ſweet, and grave at leaſt two or three Notes deeper than it was before; as was obſerv'd by ſome of the *Society* then preſent.

Experiment VIII.

An Account of an Experiment, ſhewing that an Object *may become viſible through ſuch an Opaque Body as* Pitch *in the Dark, while it is under the Circumſtances of* Attrition *and a* Vacuum.

THIS *Experiment* affords a ſignal Confirmation of another formerly made, and differs only in the *Matter* made uſe of. I before uſed Sealing-Wax, but now made choice of Pitch, which I ſerv'd as the Sealing Wax; that is, I melted it in a Globe-Glaſs, and kept 5 it turning about till the larger Half had got a pretty thick Lining of it; it was even ſo thick that a Ray of Light could no way penetrate it.

This Globe I exhauſted of its contained Air; then (being Night) I put it on the Engine *to give motion to it*; where, after it had been turn'd a *little while*, with my Hand on *that* half lin'd with the *Pitch*, I could very eaſily diſcover thro' the *tranſparent Part*, on the inward Surface of the *Pitch*, the very Shape and Lines of it, as likewiſe of my Fingers:

For the moſt eminent Parts of the Hand and Fingers that touch'd the Glaſs, appear'd all Luminous: The *other Parts* diſcover'd themſelves by the *dark Intervals* they made between the *enlighten'd Parts:* And when the Fingers were ſpread or clos'd, 'twas very obvious to the Sight.

Now, after a *ſmall Quantity* of Air was let in, the *Light* diſappear'd on the inſide of the *lined Part* (but not on the other) which began to diſcover it ſelf more and more on the *Outſide*; tho' even in *Vacuo* there was always a Light *attended on the Touch of thoſe Parts* that were moſt contiguous to the Glaſs: But now a *Circle of Light* would diſcover it ſelf juſt on the Edge of the *Pitch* which *ſeparated* it from the *tranſparent Part*; as likewiſe another Ring of *Light* ſomewhat nearer to the Axis of the Glaſs, but *both theſe*, when the Hand was apply'd to the *under Part:* For when it was remov'd to the contrary, no ſuch Appearance enſu'd. The *tranſparent* Half of the Glaſs was in all Circumſtances as in *former Experiments.* When all the Air was let in, the Electricity of the Glaſs in *all its Parts*, the *Lin'd* as well as the *Tranſparent*, performed much alike. The Threads ſeem'd to be attracted every where with equal Vigour.

To conclude; This, and the foremention'd Experiment of the Sealing Wax, plainly diſcover a *tranſparent Quality* in ſome Bodies (we call *Opaque*) under ſuch and ſuch Circumſtances: Bodies which are really Opaque, have hitherto been thought to continue always ſo. It was never ſo much as ſuſpected, that they could exchange *that Quality* for the *contrary one*, and then come back from that *contrary one* to their old State again: That they could paſs from Opaque to Pellucid, and from Pellucid to Opaque; at one time admit, and at another time oppoſe the Paſſage of Light: And all this by a meer Change of *external Circumſtances.* This Property, I ſay, is as new as 'tis real and ſurprizing: And the bare Conſideration of ſo very unlikely and unexpected a thing, may be a ground of Encouragement to hope, that ſome other odd Properties of Bodies, by ſome lucky Trials, may hereafter (as this has done) ſurprize us with a Diſcovery of themſelves.

I ſhall only add, that what is ſaid towards a Reaſon of ſuch an Appearance in the *Experiment* of the Sealing Wax, I think, is very applicable to this; to which I refer. See Pag. 167.

Experiment IX.

An Account of an Experiment, touching an Attempt *to produce Light on the Inſide of a Globe-Glaſs lin'd with melted Flowers of Sulphur, as in the Experiments of Sealing-Wax and Pitch.*

NOtwithſtanding Sealing-Wax and Pitch afford ſuch ſurprizing Phænomena, rendring the Form of Bodies viſible thro' their Opaque Subſtances, under the Circumſtances of a Vacuum and Attrition; yet there are other Bodies by which very different Effects will be produc'd; of which I ſhall give you a very remarkable Example: And that is in Flower of Sulphur, or Sulphur Sublim'd. About half a Pound of this Preparation I melted in a Ladle, and pour'd it into a Globe-Glaſs; and uſed it in all reſpects as in the other Experiments. And when it was exhauſted, and Motion and Attrition given, I expected as before, to have ſeen a Light on its Inſide: But all that we could do had no manner of effect on it, in relation to ſuch an Appearance, neither when it was exhauſted, nor when replete with Air: There was nothing to be obſerv'd but a very ſmall weak Light, which after long rubbing, ſhew'd it ſelf in that part where the Hand touch'd the Glaſs. But when I came to look upon it, I found the Sulphurous Lining all in a Body diſengag'd from the Concave Surface of the Glaſs. As to the Electricity of the Globe lin'd with this ſort of Matter; after the Attrition of it had been continued for ſome time, and the Glaſs was become pretty warm (at the ſame time full of common Air) the Hoop of Threads was held over it; but the Attraction was very inconſiderable on the the lin'd part, though on the tranſparant ſide Threads were pretty vigorouſly directed; yet not with that force and ſtrength, as when the Glaſs is perfectly clear within, as this was not; becauſe the Fumes of the melted Sulphur adhering to it, made it appear ſomewhat Cloudy.

A Repetition of the foregoing Experiment *with Common Sulphur.*

I Took a Quantity of Common Sulphur, nearly equal to what I had uſed before of the Flowers; which having melted as before, I pour'd it into another Globe-Glaſs, which I us'd in all reſpects as the former. But when I had exhauſted it, and given the uſual Motion and Attrition, the Effect was ſo ſurprizingly different, that one would ſcarce think it ſhould proceed from the ſame ſort of Body. For the Figure of my Hand and Fingers appear'd not only on its inſide, (tho' more Faint and Pale than in the Experiments of Sealing-Wax and Pitch) but on its outſide there appear'd a brisk Purple Light, ſo beautiful and agreeable to the Eye, that it was very pleaſant to behold. The Strength of this Light may be judged from hence, That the Lines of the Palm of my Hand, which being near the touching Parts, were eaſily diſcoverable by it; and were a ſmall Print plac'd at the ſame diſtance, I queſtion not but it would be legible without any great difficulty. And as this common Sulphur differ'd vaſtly in that part of the Experiment already related, from the former, ſo likewiſe, in the latter; for when the Hoop of Threads came to be held over it, (under the ſame Circumſtances as in the other) they were directed toward it as vigorouſly as in any Experiment heretofore made. The Parts lin'd and tranſparent perform'd much alike; if there was any difference, it ſeem'd to incline to that part lin'd with the Sulphur. Likewiſe, in this Experiment as in the laſt, the Sulphur was looſen'd and ſeparated from the Glaſs that contain'd it: Which therefore cannot be urg'd, as any ways conducible to the Unſucceſsfulneſs of the former.

A Repetition of this laſt Experiment, with a larger Quantity of Sulphur.

INTO a Globe-Glaſs of the ſame ſize of the former, which was about five Inches Diameter, I pour'd about two Pounds of melted Sulphur: This, when cold, contracted it ſelf, and became looſe from every part of the Glaſs, as in the former Experiments: The Sulphur cover'd more than half the inward Surface of the Globe, and its thinneſt part was about half an Inch in thickneſs. Towards the Axis it appear'd to be more than a full Inch in Subſtance. This Glaſs, when exhauſted of its Air, was uſed in every thing as the former. The Light produc'd was very conſiderable, I mean, that on its outſide, and attended with the ſame Colour and Vivacity as before ; nor was that leſs vigorous on its inſide. Comparing it with the former, notwithſtanding the thickneſs of the Lining, it was at leaſt four times greater ; but the Figure of the Fingers was now not ſo diſtinguiſhible as in the other. But on the part near the Axis (as I hinted before) where the Subſtance of the Sulphur was much the greateſt, no Light was produc'd ; which may be attributed in a great meaſure to the ſlowneſs of the Motion, and the weakneſs of it there, in compariſon with that which is made more remote from it, where it was that the Light was ſeen within.

What farther is obſervable, was, that the Light which was viſible on its outſide only, appear'd to be produc'd between the inward Surface of the Glaſs and the Convex Surface of the Sulphur ; the Sulphur being looſe from it, gave liberty for the Air to be taken from thence, as well as from the other Parts: The Light which was there produc'd, being reflected by the hard, poliſh'd, and nearly contiguous Body of Sulphur, ſeems to me to be the Reaſon why it appear'd with ſo much vigour.

This outward Light would ſometimes break into Branches all over the lin'd part of the Globe, in as odd, and as pleaſant a manner, as what has been taken notice of in former Experiments, with the large Globe Glaſs, upon letting in a little Air.

And what farther occurr'd in this Experiment, was, that when the Attrition was ceas'd, but the Globe continuing its Motion, abundance of Sparks of Light would appear all round it, and continue ſo to do for ſome time, without any freſh Attrition. I cannot conclude without taking ſome notice, that in the preceding Experiments formerly made on Sulphur, I us'd the ſame ſort as in the firſt of theſe; and had it been my Chance to have happen'd on the common ſort, I doubt not but the Succeſs of it would have been different from what is there related, which I hope to try at one time or other.

Coroll. Hence we may ſee what *Remarkable Changes* may be produc'd in Bodies, with reſpect to their *Electrical* and *Luminous* Qualities, by their different *Management* and *Preparation*: As here 'tis plain that *common* Sulphur, which is plentifully endow'd with *both* theſe Qualities, by undergoing the *Chymical* Fire (which ſublimes it into *Flowers*) is almoſt totally depriv'd of them both.

Perhaps by *other* management of the ſame Body, this Loſs might be repair'd again. And 'tis poſſible there may be *Chymical* Operations, which inſtead of impairing, may *Improve* and Heighten theſe wonderful Qualities of Bodies: Nay, for ought that I know, may as well *give them a new*, where they never were at all; as to take them away, where they once were in a great degree of Perfection. The Powers of Nature are not to be determin'd beforehand by *Demonſtration*, but to be ſearch'd out by *Obſervation* and *Experiment*. And as theſe Trials have open'd the way to *ſomething that looks with a very promiſing Aſpect*; ſo we hope by degrees to perſue them with ſome good Succeſs.

X.

Experiments *concerning the Time requir'd in the Deſcent of Bodies of different Magnitudes and Weights in Common Air, from a certain Height.*

TO make theſe Experiments accurately, I deviſed the following *Apparatus*, to account exactly for the time of the Bodies deſcending. At the Height from which the Balls were to be dropp'd, I fix'd a Contrivance in form of a Trough, in all about Four Feet long; and the End of it, on which the Balls were laid, was looſe, ſwinging on two Pins at the Extremity of it. This looſe End was ſupported by a thin Piece of Board, which ſlid under it through a Groove from the other Part of the Board: To this ſliding Board was fix'd a String which related to a ſmall Wire that reach'd to the Bottom of the Deſcent, where it (the Wire) had a Communication with a Contrivance, to give motion to a Pendulum which beats half Seconds. Now, when this ſliding Board (juſt mention'd) was *drawn* from under that Part of the Trough on which the Balls were plac'd, the Strings thereby became ſo much ſhorten'd, as to move the Limb of that Contrivance at Bottom, which dropt the Pendulum at the ſame Inſtant of time, as the Balls began to deſcend.

EXPERIMENT I.

The Firſt Experiment I made, was with *Two Balls:* One of them a *thin Glaſs Bubble*, fill'd with *Quickſilver*, its Diameter eight Tenths of an Inch, and its Weight 840 Grains: The other *Ball* was of *Cork*, whoſe Diameter was 2 Inches 2 Tenths, and its Weight 120 Grains. When theſe Balls were dropt, the Pendulum made Eight Vibrations juſt as the Quickſilver Ball ſtruck the Ground; and eight more were repeated before the Cork arriv'd at the ſame Place. The Pendulum vibrated half Seconds preciſely.

EXPERIMENT II.

I took a Quickſilver Ball, much of the ſame Diameter and Weight as before: The other was a Thin Glaſs Bubble, its Weight 493 Grains, its Diameter Four Inches Three Tenths.

For theſe, when they came to deſcend, the Pendulum made juſt ſo many Vibrations as in the laſt Experiment; that is, the Quickſilver Ball ſtruck the Ground at eight Vibrations, and the other juſt at the End of Sixteen.

EXPERIMENT III.

The Quickſilver Ball that I made uſe of in this Experiment, was likewiſe much of the ſame Weight and Diameter as before: The other Ball was of Glaſs, whoſe Weight was 535 Grains; its Diameter one way meaſur'd Five Inches ¼, and its oppoſite Diameter but Five Inches. Upon the Deſcent of theſe Balls, the Pendulum made but one Vibration more than in the other Experiment; that is, the Quickſilver grounded exactly at Eight Vibrations, and there were Nine more before the other Ball arriv'd at the ſame Place.

Theſe Experiments were made from the Top of the *Cupulo* of St. *Paul's*, *London*; from whence to the Floor, on which the Balls were dropt, meaſur'd near 220 Feet. It is to be obſerv'd, that the Quickſilver Balls made no ſenſible Impreſſion on the Floor on which they deſcended (which at that time was cover'd with Deal-Boards) notwithſtanding their Weight and Velocity of Deſcent.

The following Experiments on the Deſcent of Bodies in Air, were made in the ſame manner, at the Place before mentioned, anſwering very exactly with the former.

Quickſilver Balls.				Large thin Glaſs Balls.		
Weight in Grains	Diam. 10ths of Inch.	Time of falling in half 2ds		Wt. in Gr.	Diam. Inch 10ths	Time of falling in half 2ds.
908	.8	8		510	5 .1	17
993	.8	8	a little leſs.	642	5 .2	16
866	.8	8		599	5 .1	16
747	.7½	8	a little more.	515	5 .0	16½
808	.7½	8		483	5 .0	17
784	.7⅓	8	a little more.	641	5 .2	16

Theſe Experiments were made *June* 9. 1710. at which time the Height of the Quickſilver in the Barometer was 29.7 Inches, and the Thermometer 60 Degrees above the Freezing Point.

Note, That the Quickſilver Balls, and the large thin Glaſs Balls, were dropt together as they are ranged in their ſeveral Lines.

XI. Ex-

XI.

Experiments *concerning the Effects of* Air *paſs'd through red hot Metals*, &c.

IN order to find what Effect ſuch a *Medium*, as Air paſſed thro' red hot Metals, might have on the Lives of Animals, I contrived the following Method. I took a large Receiver open at top, in Diameter about four Inches, which was covered with a Braſs Plate and wet Leather, as uſual in Glaſſes of ſuch a make. To this Plate at top (which had a ſcrew with a ſmall Perforation) belonged a Cock, and from that Cock proceeded a ſmall hollow Wire, about three Feet in Length: That End of this hollow Wire, which was remote from the Receiver, was put into a hollow Piece of Caſt Braſs, pretty thick in ſubſtance, but the Hole was not quite through: And the Hole being larger than the ſmall hollow Wire, it wedg'd into the ſame with pieces of Steel Wire, till the Caſt Braſs was fill'd as full as it could contain. In this manner it was put into a Charcoal Fire, and there it lay till it was thorougly Red hot. The Receiver being then exhauſted of its Air, the Cock on the upper part of it was turn'd, which gave liberty for that Air only, which of neceſſity muſt paſs thro' the red hot Metals to ſucceed. This Air firſt paſſing down thro' the ſmall Ducts between the red hot Wires, before it could come to enter the Red hot hollow Braſs-Wire, muſt of neceſſity ſuffer or undergo ſuch a Change, as Fire, or the Fumes of ſuch red hot Metals would give it.

When the Receiver was fill'd with this Air, and had ſtood ſome little time, the Braſs Cover was taken off, and a pretty large Cat immediately plung'd into it: The Cover being laid on again, the Cat immediately fell into Convulſions, and in leſs than a Minute appeared without any ſign of Life. Then being taken out of the Receiver and laid on the Floor, ſhe continued as Dead; but in leſs than a Minute of time ſhe began to diſcover Life by motion in her Eyes, and after 2 or 3 hideous Squalls, ſhe began to recover apace; but was very fierce, and ſpit and did fly (as well as her Weakneſs would ſuffer her) at any one that offer'd to touch her; and it ſeem'd hazardous for any one then to attempt it.

But after half an Hours time, or thereabouts, as her Strength and Eaſe recovered, ſo her former Temper encreaſed upon her ſuffering herſelf to be handled without any ſign of fierceneſs, as before.

As to the Effect, which the ſame ſort of Factitious Air has upon Flame, take as follows.

I no ſooner came to plunge a lighted Candle into it, but it was immediately extinguiſh'd: And this I ſeveral times obſerv'd, that when the Candle was ſlowly immers'd, ſo much of the Wick (which before was lighted) as came but within the verge of the Glaſs, died; and ſo the reſt ſucceſſively, as it deſcended to the ſame place: And this upon ſeveral Repetitions anſwer'd much the ſame. But in ſome time, as the common Air came to mix with it, one might plunge the lighted Candle lower and lower, before it did go out, till at laſt it would remain burning at bottom.

As to the Elaſticity and Specifick Gravity of the foremention'd *Medium*, I ~~have~~ made ſeveral Trials, (and I think very accurate) but find it no ways differing from Common Air, in reſpect to thoſe Properties.

Hence it follows,

That the foregoing Effect, is no ways aſſiſted from any Imperfection or Defect in the laſt mention'd Properties: Therefore the following Queries ſeem to offer themſelves.

Query 1. Whether Air it ſelf may ſo ſuffer in its own Nature by any ſort of Fire, as to be diveſted of the Power of ſubſiſting Life or Flame: Or, *Query* 2. Whether the *Effluvia*, or Steems, proceeding from the red hot Metals, which the Air may take along with it in its Paſſage near them, do not very much contribute, if not wholly occaſion the Effect.

If the latter takes place, I preſume, it may in ſome meaſure be apply'd to account for the Effect, that the Damps, or Steems, which ariſe from Subterraneous Caverns, impregnated with Metalline *Effluvia*, have on the Lives of Animals: And yet at the ſame time, the ſame Air may ſuffer no Change in reſpect to its other Properties; I mean, its Elaſticity and Specifick Gravity, in compariſon with other Air in the ſame Region.

Experiments *concerning the Effect of the Air paſs'd through a Degree of Heat, equal to that of boiling Water.*

I Contriv'd a Braſs Box, about Four Inches long, and an Inch and half over. At one End, which I ſoder'd up, I fix'd two ſmall Braſs Tubes; one of which went through, and reached the remoter End nearly: The other Tube was but juſt inſerted in it; but each of them long enough to reach ſufficiently above the Surface of the Water in which they were to be put.

Theſe Tubes were to convey the Air into a Receiver exhauſted of its Air: It paſs'd firſt into that Tube which nearly reached its oppoſite End, and ſo into the other which lead to the exhauſted Receiver. But the Box, with that Part of the Tube that was within it, was firſt preſs'd full of Braſs Duſt; which I had the conveniency to do, by means of a Braſs Cap, which ſcrew'd on to the End, not before mentioned. This Braſs Duſt I moiſtned with a little Water, thinking thereby to exert a more than ordinary Steam, or *Effluvia*, from the Metal, which the Air might take along with it, as it paſs'd through ſuch ſtrait and narrow Avenues, as it muſt do between the Braſs Duſt.

In this manner it was put into the Water when cold, and continu'd in it till it had boil'd a conſiderable time; by which means it muſt in all its Parts, be of the ſame Degree of Heat (at leaſt) as the boiling Water. Thus it was taken out, and apply'd to the exhauſted Receiver; where, upon turning a Cock, I gave the Liberty for that Air only to paſs into it, which muſt ſucceed through the Braſs Box and Duſt, under the Circumſtances before mentioned.

When the Receiver was full of this Air, the Cover was taken off, and a lighted Candle plung'd into it, where it continu'd burning, even at the Bottom, as if it had paſs'd through no ſuch *Medium*, but had been full of Common Air. I took that Method to try it, believing the Flame of a Candle to be the moſt tender Way of diſcovering a Change in Air.

Afterwards I *repeated* the *ſame Experiment* over again, with dry Braſs Duſt inſtead of the former; but the Succeſs was the ſame. Therefore it ſeems to me, that ſuch a Degree of *Heat* as that of *boiling Water*, is not ſufficient to cauſe any conſiderable Change (if any at all) in the Air; nor ſuch a Degree of *Heat*, able to ſtrike any injurious or ſuffocating *Effluvia*, out of the *Metalline Particles*.

Some other Experiments on the foregoing Head.

THE paſſing Air through a red hot Glaſs Tube into an exhauſted Receiver, had no manner of influence on a Sparrow put into the ſame: But upon paſſing of Air through red hot Charcoal, before it enter'd the Tube that convey'd it into the exhauſted Receiver, the forementioned Animal, in that *Medium* in about a quarter of a Minute, gave ſigns of preſently expiring; but being taken out at the ſame time did recover, and continued living and well for ſome Days after. Yet it was concluded, had the Bird's continuance in the Receiver been but double that time, her recovery would have been very doubtful. I have likewſe try'd Air paſs'd thro' the Flames of Spirit of Wine, and Oil of Turpentine:

The Effect was much the ſame as to the Spirit of Wine, the Flame of a Candle being immediately extinguiſh'd upon its being plung'd into it: But the Air which paſs'd through the Flame of the Oil of Turpentine took ſome Unctious Fumes along with it into the exhauſted Receiver; which Fumes, upon the near approach of a lighted Candle, ſuddenly took fire, and continued to burn on the upper Surface, till they were ſtifled by covering cloſe the Receiver: And upon ſeveral Repetitions it anſwer'd much the ſame, till the whole quantity of Fume was conſum'd.

XII.

A Deſcription of the Apparatus *for making Experiments on the Refractions of Fluids : With a Table of the Specifick Gravities, Angles of Obſervations, and Ratio of Refractions of ſeveral Fluids.*

THE whole *Apparatus* is fix'd on a Table, parallel to its Surface. On one and the ſame *Axis* is fix'd a Sextant, of a *Radius* of 4 Feet, and a moving Limb to bear the Object. The Sextant is divided into Degrees and Minutes by a Diagonal, and remains always fix'd. The Object, which is plac'd on the moving Limb, is ſeen parallel with the Table when obſerv'd through the Priſm, and at no Degrees on the Sextant ; but when any Traſparent Liquid is put into the ſame, the Object muſt be elevated till it appears to the Eye: Then obſerving how many Degrees and Minutes the *Index* on the Limb cuts on the Sextant, we note it, and call it the Angle of Obſervation. Thus for different Liquids you have different Elevations of the Object, as you will find by the following Table. The Sight-Slit (if I may call it ſo) is compoſed of two Pieces of Box-Wood, plan'd parallel to one another: Theſe Pieces are ſeparated only by three ſlender Slips of common Cards; and with that intervention are ſcrew'd down one upon the other, exactly parallel with the *Axis* of the moving Leg and Sextant. The Priſm, thro' which it directs the Sight, is plac'd pretty near it, and conſiſts of an Angle of $44^d. 54'$. which Angle is fix'd Perpendicular to the Plane of the Table, its upper ſide being parallel with the ſame. The Object is a Piece of white Paper, in form of a Croſs, paſted on a black Board, and is fix'd at the end of the moving Limb, which is in length about ſeven Feet from the Sight; its Diameter is about $2\frac{1}{2}$ Inches, which juſt comprehends the Sight through the Slit; ſo that when the Object is wholly within view, we conclude the Obſervation to be exact. With this *Apparatus* the Experiments are made as well by Candle-light as Day-light, (the Preſence of the Sun Beams being no ways neceſſary) and I think they may be depended on as pretty Accurate. I have taken the Specifick Gravity of the ſeveral Liquids, where I could obtain a ſufficient quantity, as appears by the Table: So that if any Perſon ſhould have the Curioſity to repeat theſe Experiments, he muſt expect a different Angle of Obſervation, if the Specifick Gravity agree not with the Table; for ſometimes it happens, that Liquids of the ſame Denomination are not always of an equal

Goodneſs; and conſequently will have a different Specifick Gravity and Refraction.

The Chriſtalline Humour of the Ox Eye I preſs'd into the Angle of the Priſm, whereby it received the Form of it, and gave the Angle of Obſervation as ſpecified in the Table. I could not ſee the common Object thro' it, but was forc'd to make uſe of a Candle for that purpoſe; the Flame whereof appeared very broad, at leaſt five or ſix Inches, nearly in the Form of a Half Moon: But what ſhould occaſion ſuch a Change of Figure, I cannot at preſent determine. Of all the Fluids I have try'd, I find nothing to Refract a Ray of Light leſs than Water; yet there are ſeveral other Liquids which make the ſame Angle. I obſerve Oil of Bees-Wax to be the lighteſt Fluid, and Butter of Antimony *per Deliquium* to be much the heavieſt: The difference of Specifick Gravity between theſe two Bodies, is as 662 is to 1976, that is, nearly, as one to three: And the Ratio of their Refractions but as 10000 is to 6885 Bees-Wax, ſo is 5941 Antimony to the ſame *Radius*; that is, as one to 1. 16, or thereabouts. Likewiſe Oil of Vitriol is in Specifick Gravity to Oil of Saſafras, as 1510 is to 898; yet the *Ratio* of Refraction of the lighteſt is moſt conſiderable, being in proportion as 10000 is to 6475 Saſafras; ſo is the ſame *Radius* to 7011 Vitriol.

Thus I find, that a Body doth not Refract in proportion to its Specifick Gravity; but from ſome Quality peculiar to it ſelf; whether it be from its Inflammability, or from any different Texture, or Figure of its Component Parts; or whatever elſe it be, I ſhall, with the Application of theſe Experiments, ſubmit to this Honourable Society to determine.

	Specifick Gravities in compariſon with a Bulk of Water equal to 820 Grains.	Ang. of Obſervation. d. '	Ratio of Refraction, as 10000 is to
		16.50	7485.3
Oil of *Saſafras*	898	29.20	6475.8
Turpentine	713.5	25.25	6741.8
Bees-Wax	662	23.30	6885 4
Carrawayes	732	26.13	6696.5
Oranges	711	25.20	6741.2
Hyſop	769.5	25.10	6757.6
Roſemary	747	24.40	6794.7
Savin	789	25.30	6730.9
Origanum	752	25.00	6770.2
Pennyroyal	783	25.30	6730.9
Mint	780.5	26.00	6706.4
Spike	749	24.30	6807.3
Fennel	798	27.10	6616.5
Juniper	729	25.10	6757.6
Cummin	766.5	27.00	6627.7
Tanſey	757	23.46	6865.1
Dill	795.5	27.40	6582.7
Amber	783	26.30	6662.3
Cinnamon	828	28.40	6517.7
Cloves)	827	27.20	6606.8
Nutmegs	759	25.40	6721.4
Spirit of *Wine*	703.5	18.50	7287.9
Hartſhorn	786	17.00	7468.3
Vinegar	824.5	17.00	*As Hartſhorn.*
Sal Armoniack	794.5	16.56	7475.2

Acids,

Acids, Spirit of *Amber*	825	16,56	*As Sal Armoniack*
Oil of *Vitriol*	1510	21.56	7011.5
Spirit of *Nitre*	1166	20.50	7104.
Aqua Regis	987	19.50	7195.
Aqua Fortis	1157	20.40	7120.5
Aqua Regis from *Aqua Fortis* and *Sal Armoniack*	1034	20.10	7161.5
Butter of *Antimony*	1976	40.00	5941.3
Spirit of *Raw Silk*	916	20.30	7135
Spirit of *Honey*	716	16.50	*As Water.*
Tinct. of *Antimony*	693	18.46	7294.5
Jeſuits Bark	720	18.46	*As Tinct. of Antim.*
Balſamum Tolu	717	19.34	7219.3
Gum Ammoniacum	719	19.10	7257.3
Metals	713	18.54	7281.7
Vitreous Humour of an Oxe's Eye		16,50	*As Water:*
Chryſtalline Humour of an Oxe's Eye		24.10	6832.7
White of an Hen's Egg		17.40	7401.3
Jelly of Hartſhorn		17.50	7384.7
Human Saliva		16.50	*As Water.*
Human Urine		17.05	7451.9
French Brandy		18.20	7338.6

Oil of Turpentine ſtrongly ting'd Green, with Filings of Braſs, no ways alters its Refraction.

XIII. Expe-

Experiment XIII.

An Account of the Repetition of an Experiment *of the late Dr.* Hooke's, *concerning* two Liquors, *which, when mixt together, will poſſeſs* leſs Space, *than when* ſeparate : *With another* Experiment *confirming the ſame.*

THE Experiment related by the late ingenious Dr. *Hooke*, in one of his Papers (deliver'd to me by Mr. *Waller*) is concerning Two Liquors, which, when mix'd together, would poſſeſs leſs ſpace than when ſeparate ; which he calls a *Penetration of Dimenſions:* And adds further, That this *Penetration* is the Cauſe of *Heat*, of *Fire*, of *Flame*, of the *Power of Heat*, *Fire*, and *Gunpowder*, and ſeveral other *Phænomena*, which ſeem to be moſt prodigious and wonderful in Nature.

And ſince the *Experiment* ſeem'd ſo conſiderable, as to account for ſeveral ſurprizing Operations in Nature, I thought it very worthy an Examination, by a *Repetition* of the ſame.

Accordingly, I procur'd a *Bolt-Head* (ſuch as he there deſcribes) with a long ſmall Stem, which I fill'd nearly full of Common Water. The Stem was mark'd into ſeveral Diviſions, on a Piece of Paper paſted along it; by which means I diligently obſerv'd the Height of the Surface of the Water.

Then pouring as much of it out as fill'd a certain Meaſure, which being thrown away, I fill'd the ſame Meaſure as nicely as poſſible, to the ſame Height, with ſtrong Oil of Vitriol; which I return'd into the *Bolt Head*, in the room of ſo much Water taken from thence.

Upon the mixing of theſe Liquors enſu'd a pretty ſtrong Ebullition; and abundance of Airy Particles were viſibly extricated, and the Surface was not ſo high in the Stem conſiderably, as when it was poſſeſs'd only by Water.

But here I muſt take notice, that two or three Drops of Oil of Vitriol were accidentally ſpilt, in putting it into the *Bolt Head*; but yet the Experiment was very manifeſt, in the gradual Decreaſe of the Dimenſions of the Liquors.

And 'tis to be obſerv'd, That altho' they became very warm, yet, contrary to the Nature of moſt Liquors in ſuch a State, they continu'd to poſſeſs leſs and leſs Space; which was viſible by the ſinking of the Surface in the Stem of the *Bolt-Head*; and in about half an Hours time, it had deſcended above an

Inch: And when I viſited it on the *Monday* following (for the Experiment was made on the *Thurſday*) I found it had ſubſided at leaſt two Inches below the Mark I left it at.

Now, whether the *Ebullition* produced by the Heat, might not evaporate *that Quantity*, which it ſeem'd to loſe in Space; or whether, in ſo many Hours time, as it had been ſince the *Experiment* was made, there might not be ſuch an Evaporation of the Parts of the Fluid, as to become equal in Bulk to the *Quantity* of the diſpoſſeſs'd Space; and whether it was ſo or not, I gave my ſelf the ſatisfaction after the following manner.

Into an upright Glaſs that would hold about Three Ounces of Water, I put a Quantity of the ſame Fluid equal to 885 Grains.

Into another Glaſs, of the ſame form, but ſmaller, I put a Quantity of *Oil of Vitriol*, equal to 456 Grains; which, with their reſpective Glaſſes, I weighed all together in a nice Balance: After which I put the *Oil of Vitriol*, Glaſs and all, into *that* which held the *Water*; where immediately a very great *Ebullition* began, and the Glaſs that contain'd them became ſo hot, as to be but juſt endur'd in the Hand.

I found, in two Minutes time it had loſt of its Weight about Two Grains: And at the End of an Hour, or better, it had decreaſed in all but Six Grains and a half; by that time the Conflict was wholly ceas'd, it being then nearly reduc'd to the Temperature of the outward Air.

After that, I weighed them at ſeveral times, but found them in the ſame ſtate, as to their Weight, as laſt mention'd.

I continu'd them in the Scale till the next Morning, when I likewiſe could diſtinguiſh no manner of Alteration in the forementioned Weight.

From whence it plainly appears, that the Decreaſe of Bulk upon the mixing of thoſe Liquors, does not proceed wholly from an Evaporation of their Parts; ſince by the laſt Experiment, the Evaporation continu'd no longer than the Fermentation laſted; but the Decreaſe of the Bulk of the Bodies ſeems not to be perform'd all at once, or in ſo ſhort a time, as may be taken notice of in the firſt Experiment.

Experiment XIV.

An Account of an Experiment, concerning an Endeavour to produce Light *through a* Metallick *Body, under the Circumſtances of a* Vacuum *and* Attrition.

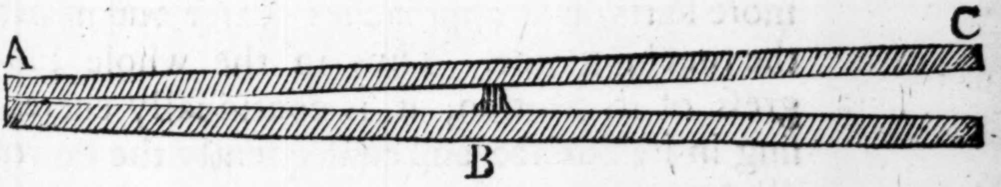
IT may be remembred, what ſucceſs I had in producing Light through Bodies, ſuch as Sealing-Wax, Pitch and common Sulphur; which gave me ſome probability, that under the ſame Circumſtances I might likewiſe make ſome ſuch Diſcovery thro' a Metallick Body. Accordingly I cauſed a Glaſs Hemiſphere to be made very ſtrong: To this Hemiſphere I procured another, of Burniſh Braſs, exactly made, to fall with its Brim about an Inch within the Glaſs that I might the better cement them together; which I did ſecurely from any ingreſs of the Air in that part. Thus, when joined it became nearly a Globe; only, the Diameter thro' its *Axis*, was ſomewhat more than its tranſverſe Diameter, which was a Diſadvantage to its Strength, as the Sequel of this relation will diſcover. In this manner I exhauſted all its Air, at leaſt nearly ſo, and then put it on the Machine to give it a circular Motion, as uſual in ſuch Experiments. I applied my Hand to the Braſs Hemiſphere in Motion, but no Light could be diſcover'd within: I then rubb'd it with a Deal Stick, but the Succeſs was the ſame. Afterwards, I applied a piece of Sealing Wax, which has in it ſelf a very Electrical Quality: This Wax, rubbing roughly on the Braſs, ſeem'd to ſhake the Parts of it; nevertheleſs, there did not any the leaſt glimpſe of Light appear. I then held the Flame of a Candle to the Braſs in Motion, which ſomething more than warm'd a Circle on it; hoping by this means to excite or obtain ſome Diſcovery from it. Yet, notwithſtanding a ſmart Attrition was made on that part, it was altogether unſucceſsful. Being tir'd, I let in the Air, and ſuſpended my farther Trials till the following Night. At which time, when I had exhauſted the Air from within the Globe, I began the Attrition with a Coal Cinder, which being ſomewhat rough I thought might ſhake the parts of the Metal, and put them into ſuch a State or Mode as to exhibit an Appearance of Light: But this and whatever elſe I then did try, was to no purpoſe. In this exhauſted State I left the Globe on the Engine, to conſider a little what farther Trials to make; with what Bodies and in what manner to proceed with them: But to my great ſurprize, in about an Hour after (being in the next

Room) I heard a Noiſe almoſt as loud as a Musket when fir'd ; and I immediately coming into the Room, found the Globe broken all to pieces (I mean the Glaſs half of it) and the Braſs Hemiſphere on the Ground ; which I took up, and found ſeveral bruiſes it had received from the violent ſtrokes of the broken Glaſs, which had diſperſed it ſelf in pieces all over the Room. A large Looking-Glaſs, at leaſt three Yards diſtant from it, was crack'd almoſt from top to bottom, and quite croſs the middle, by a blow it receiv'd from a Fragment of it ; for where it ſtruck the Glaſs, the Cracks proceeded from it every way like ſo many *Radii* drawn from a Center. Thus were the Experiments ended ; and, as I hinted before, this Accident, I believe, proceeded from the unconformableneſs that the Figure of the compounded Globe, had to a perfect Sphere, altho' it did not differ ſo much to ſight, as to make me ſuſpect any ſuch Conſequence. From theſe Experiments I may ſafely conclude, that if there be any ſuch Quality as Light to be excited from a Braſs Body, under the forementioned Circumſtances, all the Attritions of the ſeveral Bodies uſed for that purpoſe, have been too weak to force it from it. And indeed, conſidering the cloſeneſs of the Parts of Metal, and with what firmneſs they adhere, entangle, or attract one another, a ſmall degree of Attrition is not ſufficient to put their Parts into ſuch a Motion, as to produce an Electrical Quality; which Quality, under the forementioned Circumces, I take to be the Appearance of Light in ſuch a *Medium*.

Experiment XV.

An Account of an Experiment touching the Direction of a Drop of Oil of Oranges, between two Glaſs Planes, *towards any Side of them that is neareſt preſs'd together.*

THE Glaſs Planes which I us'd, were about ſix Inches ſquare; and being very clean, a Drop or two of Oil of Oranges was let fall on the lower Plane, ſuppoſe at B; then the upper Plane was laid on it, ſo near as to touch the Liquid, that it might become contiguous to both their Surfaces.



Thus the Planes being made to touch one another at the Side A, and open'd at the Side C, as in the Scheme above, the lower Plane lying Parallel with the Horizon, the Drop of Oil would immediately move towards the touching Side of the Planes; and when it was arriv'd there, it was but reverſing the Angle, and the Drop would return from A to C; and after the ſame manner it might be directed to any Side or Part of the ſame. Moreover, if the Planes were elevated 8 or 10 Degrees at A, yet would the Drop aſcend towards the Side A, tho' not ſo ſwift, as when the Planes were in the forementioned Poſition.

It was farther to be obſerv'd, that the nearer the Drop approach'd the touching Side, ſo would the Velocity of its Motion be encreaſed: The reaſon of which ſeems very plain, allowing the Aſcent of Water in ſmall Tubes, and between the Surfaces of nearly contiguous Planes, to be explain'd from the Power of Attraction, that one Surface has to another at ſuch a nearneſs (as I ſee no reaſon to doubt it:) For the Drop of Oil moving on towards the contiguous Surfaces, comes to enlarge its Space, and touches the Planes in more and more Parts, as it approaches nearer and nearer the touching Side. Thus in the whole Progreſs of its Motion, it is continually encreaſing in its Surface, and conſequently the Power of Attraction muſt encreaſe in proportion to that Surface: ſo that the Celerity of its Motion on muſt neceſſarily be augmented. This Experiment ſeems very powerfully to confirm the Experiments made before on the ſame Subject, from the gradual Increaſe of the Motion of the Drop; repreſenting thereby the ſeveral Appearances of the Aſcent of Water in different ſiz'd Tubes, or between Planes, whoſe Surfaces are placed at different diſtances, the ſlower Motion repreſenting thoſe Experiments made in larger, and the ſwifter in ſmaller Tubes; the ſame to be obſerved in different diſtanced Planes.

I have ſince repeated the ſame Experiment *in Vacuo*, where, in all reſpects, it anſwer'd as in the open Air: Which is a plain Indication, that the Preſence of the Air has nothing at all to do in producing this *Phænomenon*.

XVI.

An Account of ſome Experiments touching the keeping of Fiſhes in Water under different Circumſtances.

THE Fiſhes made uſe of in the following Experiments were Gudgeons; which are a ſort of Fiſh very brisk and lively in the Water, and will live a pretty conſiderable time out of it. Three of them I put into a Glaſs Veſſel, to about three pints of common Water (which Fiſhes were to be a Standard to compare the others by.) Into another Glaſs, to a like Quantity of Water I put three more of them, which quantity of Water juſt fill'd this Glaſs to the very Brim; upon which I ſcrew'd down a Braſs Plate with a Leather between, to prevent a Communication with the Water in the Glaſs and the External Air: And that it might the better reſemble a Pond of Water frozen over (on which Account this Experiment was made) I ſuffer'd as little Air as poſſible to remain on the Surface of the included Water.

The third Glaſs had a like quantity of Water put into it, as the former; which Water, firſt by boiling, then by continuing it a whole Night in *Vacuo* on the Air-Pump, was purg'd of its Air to the greateſt nicety: Into this Water alſo, I put a like Number of Gudgeons as into the other. Thus (the Fiſhes being all put into their reſpective Receivers) I apply'd my ſelf to wait the Event; which was as follows. It was about half an Hour after Ten in the Morning when I began the Experiments; and in about half an Hour from that time, I obſerv'd the Fiſhes in the exhauſted Water or Water purg'd of its Air, began to diſcover ſome uneaſineſs, by a more than ordinary Motion in their Mouths and Gills, or Reſpiration, if I may call it ſo, differing from the Fiſhes in the other Glaſſes; the included Fiſhes at the ſame time diſcovering no alteration; only I took notice that they would now and then aſcend to the Top of the Water, but ſuddenly ſwim down again: And in this State they continued for ſome conſiderable time, without any ſenſible Alteration. About 5 Hours after the laſt Obſervation, the Fiſhes in the exhauſted Water became not ſo active (upon a Motion given the Glaſs that contain'd them) as before: And thoſe Gudgeons included without any Communication with the outward Air, now began conſiderably to abate of their Vivacy; yet ſtill continued at times their Motions upward and down again. At Seven in the Evening, the included Fiſhes lay all at the bottom of the Glaſs with their Bellies upwards; nor upon ſhaking the Glaſs, could I put them in Motion, or cauſe them to ſtir their Fins or Tail; only I could obſerve a Motion in their Mouths, which ſhew'd me they were not perfectly dead. In this State they lay for ſome time: But conſidering the Experiment would not be compleat, if I did not attempt their Recovery by taking off the Braſs Cover, being very ſure they muſt have dy'd in ſome ſmall time under the Circumſtances they were then in; accordingly, I took off the Cover, and gave the Surface of the Water a free and open Communication with the External Air. At about Ten at Night, I obſerv'd them again; at which time their Recovery was ſo evident, that upon a little diſturbing the Glaſs that contain'd them, they were actually in Motion again: And at this time alſo, the Fiſhes in the Water purg'd of Air, began to appear more brisk and lively than at the laſt Obſervation. Here I cannot but take notice, that notwithſtanding the Water was purg'd of its Air to a very great degree, yet the Fiſhes put into it did not ſo much as once aſcend in it; but continued always at the bottom, as the Fiſhes did in the Common Water. At this time I left them till the next Morning; when about Eight a Clock I found them as perfectly well and lively in all the Glaſſes, as when they were firſt put in. Thoſe in the Common Water expos'd to the open Air, ſuffer'd no manner of Change during the whole Time. After this I was willing to try whether the Air had again inſinuated it ſelf into the purg'd Water,and whether that might not be the occaſion of the Fiſhes Recovery. Accordingly I put it on the Plate of my Pump, in the ſame Glaſs with the Fiſhes in it; and being cover'd with another Receiver, the Air was taken from it; yet I could perceive very little Air aſcend in it, and to me it ſeem'd much in the ſame State as when the Fiſhes were firſt put in. I continued it *in Vacuo* about an Hour and an Half; the Fiſhes almoſt all the time continued at the Top of the Water, and at that time appeared as dead; for upon letting in the Air, they ſunk haſtily to the bottom without any Motion of their Fins or Tails.

From the whole Account I obſerve;

Firſt, That Water purg'd of Air, ſo far as the Method here made uſe of, is capable to do it, renders it not altogether unfit to ſupport the Lives of Water Animals. For although when the Fiſhes were firſt put in, and for ſome Hours after, they ſeem'd to ſuffer ſome uneaſineſs; yet at length the Water became more familiar to them, or their Conſtitutions in ſome meaſure did ſo conform, as to render the Water to them, and them to the Water more agreeable: Otherwiſe I do not ſee how their Recovery ſhould follow, ſince upon examining, little or no alteration could be found in the

Circumſtances of the Water, from the time the Fiſhes were firſt put in.

Secondly, The Fiſhes included with their Water from any Communication with the External Air, plainly demonſtrate, that Common Water in its Natural State is not alone ſufficient to preſerve the lives of its Natural Animals. Hence it follows, that in Ponds, when the Water comes to be Frozen over with a pretty thick Ice, the Fiſhes in the ſaid Ponds are very likely, if not certain to periſh, upon the Continuance of ſuch a Congelation for ſome time on their Surfaces; unleſs (as in the latter part of the Experiment) the Impediment which hinder'd the immediate Contact of the Air to the Surface of the Water, be remov'd; that is, by breaking Holes in the Ice, whereby it is reſtored, and undoubtedly will perform the ſame thing as my Removal of the Braſs Plate. This is to be underſtood only in Ponds where the Water is ſtagnant; for where there are Springs, or a current of Water conſtantly ſucceeding under the Ice, the effect moſt likely will not be the ſame.

Expe-

Experiment XVII.

An Account of an Experiment, concerning the Angle requir'd to ſuſpend a Drop of Oil of Oranges, at certain Stations, between two Glaſs Planes, *placed in the Form of a Wedge.*

I Procured two Glaſs Planes that meaſur'd a *Radius* of twenty Inches each; their breadth was about three Inches: That which I uſed for the lower Plane, was plac'd with its Surface parallel with the Center of its *Axis*, and parallel with the Horizon. Thus (the Planes being very clean) they were rubb'd with a clean Linnen Cloth dipt in Oil of Oranges: Then a Drop or two of the ſame Oil being let fall on the lower Plane near the *Axis*, the other Plane was laid on it; which ſo ſoon as it touch'd the Oil, the Oil ſpread it ſelf conſiderably between both their Surfaces. Then the upper Plane being raiſed a little at the ſame end by a Screw, the Oil immediately attracted it ſelf into a Body, forming a Globule contiguous to both Surfaces, and began to move forwards towards the touching ends. When it had arriv'd two Inches

from

the *Axis*, an Elevation of 15 Minutes at the touching ends ſtopp'd its Progreſs, and it remain'd there without Motion any way. The Planes being let fall again, the Drop mov'd forward till it came to four Inches from the Center; then an Elevation of 25 Minutes was requir'd to give it a fixt Station. At 6 Inches it requir'd an Angle of 35 Minutes; at 8, of 45 Minutes: at 10, a Degree. At 12 Inches from the *Axis*, the Elevation was 1 Degree 45 Minutes; and ſo on, at the ſeveral Stations, as they ſtand in the following Table. This, after abundance of Tryals, I take to be the moſt correct, tho' the others ſucceeded very little different from the ſame. It is to be obſerv'd, that when the Globule, or Drop, had arriv'd to near 17 Inches on the Planes from their *Axis*, it would become of an Oval Form; and as it aſcended higher, ſo would its Figure become more and more oblong; and unleſs the Drop was ſmall, upon ſuch an Elevation of the Planes as was requir'd at ſuch a Progreſs of the Drop, it would be parted, ſome of it deſcending, and the reſt of it running up to the top at once: But upon a Drop that ſeparated thereabouts, I found the remaining part of it at 18 Inches, would bear an Angle of Elevation equal to 22 Degrees to balance the Weight of it. Higher than that I could not obſerve. The Planes were ſeparated at their *Axis* about $\frac{1}{18}$ of an Inch. I found but little difference between ſmall and larger Drops of the Oil, in relation to the Experiment. The Angles were meaſured by a Quadrant mark'd on Paper of near 20 Inches *Radius*, divided into Degrees and Quarters.

Diſtance in Inches from the *Axis*.	Angle of Elevation. D.	M.
2	0	15
4	0	25
6	0	35
8	0	45
10	1	00
12	1	45
14	2	45
15	4	00
16	6	00
17	10	00
18	22	00

Experiment XVIII.

The Specifick Gravities *of ſeveral* Metalline Cubes, *in compariſon with their like Bulks of Water.*

THeſe Cubes were deliver'd to me by Dr. *Sloane*, and ſuppos'd to be extraordinary in their ſeveral kinds, except the Gold. Their Workmanſhip was very accurate; and they were exactly of a ſize, altho' they differ'd a little from our Meaſure.

Six of 'em being laid on a Rule, Side by Side, meaſur'd about a Tenth more than ſix Inches; and if their Sides were changed, they ſtill made the ſame Meaſure. And it farther appear'd, that they were exact, by their Agreeableneſs in the Weight of their reſpective Bulks of Water, as may be obſerv'd by comparing them in the following Table.

Troy

Troy-Weight.

	Weight of the ſeveral Cubes in Air.			Weight of the ſeveral Cubes in Water.				In proportion to their like Bulk of Water.	
	O.	*DW.*	*G.*	*O.*	*DW.*	*G*			
Gold	9	11	8	9	00	6½	as	17.3	to Unity.
Silver	5	14	10	5	03	10½		10.421	
Copper	4	16	8	4	05	10½		8.841	
Braſs	4	05	7	3	14	9		7.81	
Lead	6	02	12	5	11	17		11.35	
Iron	4	04	17	3	13	20		7.777	

Experi-

Experiment XIX.

An Account of an Experiment touching the Aſcent *of* Water *between Two* Glaſs Planes, *in an Hyperbolick Figure.*

I Took Two Glaſs Planes, each ſomewhat more than Twenty Inches long, of the trueſt Surfaces I could procure.

Theſe being held cloſe together at one of their Ends, the other Ends were open'd exactly to an Angle of 20 Minutes.

In this Form they were edgeways put into a Trough of tinged Water, which immediately aroſe between them in the Figure of the annex'd Scheme.

See Tab. 8. *Fig.* 2.

At another time the Planes were open'd to an Angle of 40 Minutes; then the Water appear'd between 'em, as in the Scheme with that Title.

See Tab 8. *Fig.* 1.

By theſe Schemes the Proportions of the Power of Attraction are in ſome meaſure evident to the Eye: For there may be ſeen at the ſeveral Diſtances, how many Lines (which are Twelfths of Inches) the Water is elevated,

ted, and the prodigious Increaſe of them near the touching Ends.

I hope, the Tables are pretty accurate: For after many Trials, I find the Succeſſes to be much the ſame, according to the different Angles.

This Experiment was firſt made by Mr. *Brook Taylor*, as appears by his Letter to Dr. *Hans Sloane*, R. S. Secr. but he confeſſes his *Apparatus* not nice enough to diſcover exactly the Figure which the Water made between the Planes.

A Table according to the Scheme of the Planes opened to an Angle of 40 *Minutes in* Fig. 1.

Diſtances in Inches and Parts of Inches from the touching Ends.	Number of Lines elevated at the ſeveral Diſtances.
9.	1.
6.	2.
4¾.	3.
3.	4¾.
2½.	6.
2.	7½.
1½.	10.
1¼.	12.
1.	15.
0¾.	19.
0½.	28.
0¼.	50.

A Table according to the Scheme of the Planes opened to an Angle of 20 *Minutes in* Fig. 2.

Diſtances in Inches and Parts of Inches from the touching Ends.	Number of Lines elevated at the ſeveral Diſtances.
13.	1.
9.	2.
7.	3.
6.	3¾.
5.	5.
4.	6¾.
3.	9.
2½.	12.
2.	15½.
1¾.	18.
1½.	21½.
1¼.	27½.
1.	35.
0¾.	50.
0½.	76.

XX. *A*

XX.

A Deſcription of the ſeveral Strata *of Earth, Stone, Coal, &c. found in a Coal-Pit at the Weſt End of* Dudly *in* Stafford-ſhire *: By Mr.* Fettiplace Bellers, *F.R.S. To which is added, a Table of the Specifick Gravity of each* Stratum *: By Mr.* Fr. Hauksbee, *F.R.S. Communicated by Dr.* Hans Sloane, R.S. *Secr.*

I. A Yellowiſh Clay, which lyes immediately under the Turf.

II. A Bluiſh Clay.

III. A Bluiſh hard Clay; the Miners call it *Clunch*. This is one of the certain Signs of Coal. It has in it Mineral Plants.

IV. A Bluiſh ſoft Clay.

V. A fine-grained Gray Stone: It lies next the former, and is found in ſome Pits only.

VI. A Clay almoſt like the firſt, only whiter.

VII. A hard Gray Rock; with ſomething like the Impreſſions of Vegetables, but none diſtinct.

VIII. A Blue *Clunch*, like Numb. 3. with Mineral Plants in it.

VIII. +. This *Stratum* (which is the ſame with Numb. 13.) was not taken.

IX. Coal, called *Bench-Coal*.

X. Coal, leſs black and ſhining than the former, called *Slipper Coal*.

XI. Coal, more black and ſhining, called *Spin Coal*.

XII. A Coal like *Cannal-Coal*, by the Miners called *Stone Coal*. Theſe *Strata* of Coal have between each of them a *Bat*, of about the thickneſs of a Crown Piece.

XIII. A black Subſtance called a *Dun-Row-Bat*.

XIV. A hard Grey Iron Oar, called the *Dun-Row Iron-Stone*.

XV. A bluiſh *Bat*, in which the following *Iron-Stone* lies, called the *White-Row*.

XVI. A hard blackiſh Iron Oar, lying in ſmall Nodules, having between them a white Subſtance; and from thence by the Miners called the *White Row Grains*, or *Iron Stone*.

XVII. A hard grey Iron Oar, with ſome white Spots in it, called the *Mid Row Grains*.

XVIII. A black fiſſile Subſtance, called the *Gublin Bat*.

XIX. A hard blackiſh Iron Oar, with white Spots in it, called the *Gublin Iron-Stone.*

XX. A *Bat* in Subſtance much like that of Numb. XVIII.

XXI. A hard grey Iron Oar, called the *Cannoc*, or *Cannot Iron Stone.*

XXII. A *Bat*, ſomewhat harder than Numb. XX.

XXIII. A dark, grey, hard Iron Oar, call'd the *Rubble Iron Stone.*

XXIV. The *Table.Bat*, next under the *Rubble Iron-Stone.*

XXV. A coarſe ſort of Coal, called the *Foot-Coal.*

XXVI. A black, brittle, ſhining *Bat.*

XXVII. The *Heathen-Coal.*

XXVIII. A Subſtance like a coarſe Coal, but by the Miners called a *Bat*; perhaps becauſe it does not burn well.

XXIX. The *Bench Coal.*

XXX. A *Bat* under the laſt, and is as low (*viz.* 188 Feet and half) as they generally dig, tho' there is a coarſe Coal under this.

N. B. Thoſe Subſtances which divide the *Strata* of Coals and Iron Oars from each other, are called *Bats* by the Miners: They are generally black, conſiſting of a Matter peculiar to themſelves, and are of a Texture neareſt like *Marle*; tho' ſome of them are fiſſile, and others have a Subſtance not unlike Coal, mixt with them.

A Table of the Thickneſs of each Stratum, *and its* Proportion *to* Water, *or* Specifick Gravity.

Number of the *Strata.*	Thickneſs of each *Stratum.*		Proportion to Water.		Or Specifick Gravity.
	Feet.	*Inch.*			
I.	4	0	as 385	to 192	as 200 to 100
II.	5	0	296	168	176
III.	24	0	23	9	256
IV.	9	0	209	106	197
V.	4	0	583	237	246
VI.	21	0	401	192	209
VII.	75	0	683	259	243
VIII.	5	0	223	88	253
VIII +.	1	0	----	----	----
IX.	3	0	7	5	140
X.	3	0	106	72	147
XI.	4	0	147	114	129
XII.	4	0	185	143	130
XIII.	1	0	408	198	206
XIV.	0	1	204	67	303
XV.	0	3	183	72	254
XVI.	1	3	325	232	334
XVII.	0	2	781	244	320
XVIII.	2	0	305	129	236
XIX.	0	9	920	266	346
XX.	1	6	192	76	253
XXI.	0	6	675	216½	313
XXII.	1	0	428	165	290

Number of the *Strata*.	Thickneſs of each *Stratum*.		Proportion to Water,		Or Specifick Gravity.
	Feet.	*Inch*			
XXIII.	0	6	as 828	to 231	as 358 to 100
XXIV.	2	0	333	153	218
XXV.	1	0	198	154	128
XXVI	6	0	238	141	169
XXVII.	6	0	298	236½	126
XXVIII.	0	1	267	186	144
XXIX.	2	0	314	240	131
XXX.	0	6	244	133	183

By which it is evident, that the Gravities of the ſeveral *Strata* are in no manner of Order; but purely caſual, as if mixt by chance.

XXI.

An Account of Experiments concerning the Proportion *of the* Power *of the* Loadſtone *at different Diſtances.*

WIthout mentioning the many Difficulties that attend the making of Experiments of this nature, I ſhall immediately give an account of their Succeſs, and the manner of proceeding; which was as follows.

I took a Quadrant of Four Feet *Radius*; and having fix'd it to the Floor, in the Poſition of the Needle, whoſe South Point directed it ſelf to no Degrees, I then fix'd a Board (likewiſe on the Floor) in a direct Angle from the ſame, the Graduations on which Board were Three Inches diſtant from each other.

The Needle was ſuſpended on a Point ariſing from the Center of the Quadrant, from whence were meaſured the ſeveral Stations of the Magnet.

The Magnet was laid on a thin Piece of Board; under which, to one ſide was nail'd a narrow Slip of Wood, to ſlide it along the ſide of the forementioned graduated Board, whereby by the Stone might be always kept in the ſame Direction to the Needle.

The Stone that I us'd, weigh'd about ſix Pounds; was rough, and of an irregular Figure: Yet I could diſcover no Inconveniency in the Experiment ariſing from the ſame, it being, and acting at all Diſtances in the ſame Poſition as it is firſt placed on the Board: And I ſee no reaſon to doubt, but the Proportions of its Power will be regular, and agreeable to the ſeveral Diſtances; as, more than once I have obſerv'd. For when the Stone hath been differently poſited on the foremention'd thin Board, different Angles of the Needle would enſue at the ſame Stations; yet their Proportions one to another would be nearly the ſame.

My meaning is this: Suppoſe the Stone was ſo plac'd, as at 3 Inches from the Needle it would give the Needle an Angle of 90 Degrees; the Stone being continu'd in the ſame Direction at the ſeveral Stations, the Proportions of its Power one to another would be much the ſame, as if the Angle of the Needle at the firſt beginning made but 87, or even but 80 Degrees on the Quadrant: For upon a ſmall Alteration of the Poles of the Stone, ſuch diverſity of Angles will ariſe.

In theſe Experiments I made uſe of Two Needles; one of a *Radius* of 6 Inches, the other but of one Inch: Which laſt, after abundance of Trials, I found to be moſt accurate; beſides the Advantage it gave in beginning the Experiment 6 Inches nearer the Stone than the other: And from Two Feet diſtance from the ſame, it became nearly agreeable to the Angles made by the long Needle to all the farther Diſtances; as you will find by the following Tables, which were made with the ſeveral Needles in the ſame Direction of the Stone.

I meaſured the Angles by a Silk Thread ſtrained directly over the Needle to that part of the Quadrant to which it was directed; which was the beſt way I could contrive to come neareſt the Truth.

It may be obſerved from the following Tables, that the long Needle at Nine Inches from the Stone, made ſomewhat a larger Angle than the ſhort Needle at Three Inches diſtance from the ſame; that the ſhort Needle at the diſtance of Nine Inches, made an Angle of Nine Degrees leſs than the long one at the ſame place. But this odds will eaſily be accounted for, if we conſider the Diſproportions of the Needles Lengths: For the Point of the long Needle at Nine Inches, was brought within an Inch as near the Stone, as the Point of the ſhort Needle was, when but Three Inches diſtant from the ſame: The Point of the ſhort Needle at Nine Inches from the Stone, was Five Inches farther from it, than the long one at the ſame Station.

Theſe Diſproportions being conſider'd, it is no wonder ſuch difference of Angles ſhould enſue upon the Uſe of the ſeveral Needles near the Stone: For at Two Feet, and the farther Diſtances, they become nearly agreeable, as I ſaid before.

When I ſpeak of Diſtances from the Needle, I always mean from the Center of it.

Farther, it is obſervable, that the Stone at Five Feet diſtance from the Needle, made an Angle of Two Degrees with one; and with the other, of two and a half; yet upon the Abſence of the Stone, they would return to no Degrees, as at firſt: Which plainly ſhews, that the Influence of the Stone extended farther; altho' Obſervations at remoter ſtations could not eaſily be determin'd.

Experiments by the ſhort Needle.

Diſtances of the Load-ſtone from the Needle in Inches.	The ſeveral Angles of the Needle at the ſeveral Diſtances. D. '	The Differences compared one with another, at the ſeveral Obſervations in Minutes.
3	87—00	180
6	84—00	330
9	78—30	570
12	69—00	735
15	56—45	795
18	43—30	630
21	33—00	540
24	24—00	360
27	18—00	270
30	13—30	150
33	11—00	135
36	8—45	105
39	7—00	90
42	5—30	60
45	4—30	40
48	3—50	30
51	3—20	20
54	3—00	15
57	2—45	15
60	2—30	00

Experiments by the long Needle.

Diſtances of the Load-ſtone from the Needle in Inches.	The ſeveral Angles of the Needle at the ſeveral Diſtances.		The Differences compared one with another, at the ſeveral Obſervations, in Minutes.
	D.	′	
09	87	30	345
12	81	45	570
15	72	15	1137
18	53	20	1100
21	35	00	660
24	24	10	380
27	17	50	280
30	13	10	180
33	10	10	130
36	8	00	90
39	6	30	75
42	5	15	65
45	4	10	40
48	3	30	30
51	3	00	25
54	2	35	20
57	2	15	15
60	2	00	00

At greater Diſtances, and even the more remote in theſe Tables, the Power of the Stone is ſo weak, and the meaſuring the Angles at all times exactly, ſo difficult, that 'tis well if we come ſometimes within 10 or 20 Minutes of Truth: The Correction of which I ſhall wholly leave to the Determination of ſuch Gentlemen whoſe Province ſuch an Experiment as this moſt peculiarly belongs to.

A

A Deſcription of the Loadſtone *made uſe of in the foregoing Experiments.*

THIS Stone weigh'd exactly ſix Pounds, one Ounce and a quarter Averdupois-Weight. Its Form reſembl'd *Figure* 3. Its Breadth at the North Pole was four Inches; at the South Pole five Inches; the Poles running through the Stone in the direction of the prick'd Line. The Length of the ſhorteſt Side was ſix Inches and an half, of the longeſt Side ſeven Inches and half. Its Thickneſs at the North Pole was one Inch and an half, and at the South Pole one Inch.

Expe-

Experiment XXII.

An Account of an Experiment touching the Proportions *of the Aſcent of Spirit of Wine between two* Glaſs Planes, *whoſe Surfaces were plac'd at certain different Diſtances from each other.*

I Took two Glaſs Planes about 6 Inches long, and 2 broad: Theſe (being made clean) I ſeparated at each end by the Number of 32 Pieces of Braſs *Laminæ*, whoſe Thickneſs when laid one upon another, and preſs'd together by Screws, made a Diſtance between the Planes equal to (as near as I could meaſure) $\frac{1}{18}$ of an inch. Being thus prepar'd, I plung'd one End of them into ſome tinged Spirit of Wine; and after I had wetted the inward Surfaces of the Planes with it, by declining the upper end, I ſet them upright; and ſound that the Surface of the Wine between them remain'd higher than the Surface of the Wine on their Outſides, by a diſtance equal to the firſt Line C D above the Line A B, the Line A B being the common Surface, in all the Trials, on the Outſide of the Planes. (*See Plate* 8. *Figure* 5.) After this, I reduc'd the Diſtances

ces of the Planes to half the former, by taking away 16 of the Braſs *Laminæ* from each End of them; then being plunged into the Liquid, and uſed in all reſpects as before, I found the Wine to ſtand between the Planes juſt double the height it ſtood at in the former Trial; as may be obſerved by the Line EF in the Scheme. Again, removing half the Number of Braſs Pieces, whereby but 8 of them were left at each end, and uſing them as in the preceding manner, the Wine roſe between them to a Height juſt double to that in the foregoing Trial, mark'd in the Scheme by the Line GH. Thus from 8, I reduced them to 4 Pieces at each End of the Planes; then again, the Wine was ſeen to remain ſuſpended to twice the laſt obſerved Height, repreſented by the Line IK. In this laſt Trial the Planes were diſtant one from the other but $\frac{1}{128}$ of an Inch; nearer than that I could make no certain Meaſure: But I ſuppoſe, the foregoing Experiments are ſufficient to ground a Calculation upon, even of the moſt near Approximations.

XXIII. *An*

XXIII.

An Account of ſome farther Experiments touching the Aſcent of Water between two Glaſs Planes *in an Hyperbolick Curve.*

THE Figure of the Hyperbolick Curve, form'd by the Aſcending of the Water between two Square Glaſs Planes, as related in the XVIII*th* precedent Experiment, gave me occaſion to make ſome farther Enquiries; and by many Experiments I find, that the ſame Curve holds in all Directions of the Planes, the Aſſymtotes being always, one the Surface of the Water, the other a Line drawn along the touching Sides. Thus, when the touching Sides were plunged under the Surface of the Water, and the Angle *c* was depreſs'd, and made to remain lower than the Angle *a*, as in *Fig.* 4. then would be produced ſuch an oblique Curve, as may be obſerv'd in that Figure. In all the ſeveral Schemes, *a b* repreſents the Surface of the Water on the Outſide of the Planes, and *a c* the touching Sides of the ſame. Now, tho' the Curve *d d* riſes between the Planes in ſuch an

See Tab. VIII.

an Obliquity, yet does it conform it ſelf in its Figure to the Aſſymtotes, *viz. a b,* the Surface of the Water, and *a c*, the touching Sides of the Planes: For ſuppoſing the Aſſymtote *a c* to be continu'd, as in the prick'd Line, till it ſurmounts the Surface of the Water to ſuch a Height; or ſuppoſe the Planes extended in the ſame manner, then would the Water remain between them in the Appearance of the prick'd Lines, being at all diſtances from the Axis of the Curve, equal in reſpect to the Aſſymtotes; and ſo of all the reſt of the Curves, as in the Figures 5, 6, 7, 8, 9. which are the Reſult of the ſeveral Angles, made by the touching Sides of the Planes. Now, when the touching Sides were plac'd upwards, parallel to the Surface of the Water (as in *Fig.* 10.) and plunged wholly under the ſame; then, upon lifting them up, in the ſame Poſition, till the Weight of the Water between the Planes overbalanc'd the Power of their Attraction, two Curves, one from each Side of the Planes, would open themſelves, and meet each other in the middle, as repreſented in the aforeſaid Figure; where they would unite, and make a Figure as join'd by the prick'd Lines, being wider in the middle, than towards the Sides of the Planes. And it is highly remarkable, that this Curve would always break out between the Planes, at an equal Diſtance between the touching Sides and the Surface of the Water.

The ſame Figure is likewiſe produc'd between two round Glaſs Planes, *(See Fig.* 11.) the Aſſymtotes being the ſame as the former; that is to ſay, one the Surface of the Water, the other a Tangent drawn from the touching Point, parallel to the Tangent drawn from the open or oppoſite Part of Planes, being at right Angles with a Line drawn through the Axis of the ſame.

Theſe Experiments I find to anſwer the ſame in *Vacuo*, as in the open Air; ſo that that Element has nothing to do in this extraordinary Appearance.

The Planes made uſe of in the foregoing Experiments, were about 7 Inches ſquare, open'd on one ſide to an Angle of 20 Minutes or thereabouts; the round Planes were in Diameter near 3 Inches.

XXIV.

A farther Account of the aſcending of Drops of Spirit of Wine between two Glaſs Planes *twenty Inches and a half long; with a Table of the Diſtances from the touching Ends, and the Angles of Elevation.*

THE Spirit of Wine did not move between the Planes, with that Nimbleneſs as Oil of Oranges; which gave me the liberty to obſerve the Angles with more Deliberation.

The Limb on which the Planes were laid, mov'd in the Center of a Quadrant of a Four Foot *Radius*; the Largeneſs of which gave me conveniency of meaſuring the Angles with greater Accuracy: But the Diſtance between the Drop on the Planes and the Graduations on theQuadrant made it a little difficult to obſerve them both at once. Yet I believe, the following Tables may be depended on to be as true as the Nature of ſuch an Experiment (for any thing that I ſee at preſent) is capable of.

I have before, in Experiment Numb. XV. given a particular Account of the manner of making this Experiment; to which I refer.

Theſe Tables are calculated from the touching Ends of the Planes: And it is to be obſerved, that in the Table where the Planes were open'd but to an Angle of 10′, that I could not come nearer than Four Inches of the touching Ends in my Obſervations: But ſo far as I could go, ſeems to be much in the ſame proportion (as I have oftentimes obſerv'd in the courſe of theſe Experiments) with the Table where the Planes were open'd to an Angle of 18′.

Diſtances

Diſtances in Inches from the touching Ends.	Angle of Elevation	
	D.	M.
18½	0	45
16½	0	55
14½	1	05
12½	1	20
10½	1	30
9½	1	40
8½	2	00
7½	2	30
6½	3	20
5½	4	25
4½	6	00
4	7	23
3¾	8	40
3½	9	25
3¼	10	30
3	12	40
2¾	15	00
2½	18	50
2¼	23	25
2	30	00

The Planes open'd to an Angle of 18′. theplanis opened

Diſtances in Inches from the touching Ends.	Angle of Elevation.	
	D.	M.
18½	1	30
16½	1	50
14½	2	10
12½	2	40
10½	3	10
9½	3	30
8½	4	00
7½	5	05
6½	7	40
5½	10	50
4½	14	18
4	18	00

The Planes opened to an Angle of 10′.

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